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THE PSYCHOLOGICAL REVIEW.

THE INFLUENCE OF IMPROVEMENT IN ONE MENTAL FUNCTION UPON THE EFFICIENCY OF OTHER FUNCTIONS.[1]

III. FUNCTIONS INVOLVING ATTENTION, OBSERVATION AND DISCRIMINATION.

BY PROFESSOR E. L. THORNDIKE,
Teachers College, Columbia University,

AND DR. R. S. WOODWORTH,
New York University Medical College.

The functions trained in these experiments were those of observing accurately some detailed features of the words on printed pages. The subject would, for instance, practice marking every word containing the two letters *e* and *s* until he attained a considerable improvement in speed and possibly in accuracy as well. Before and after this training he would be tested in marking words containing other combinations of letters, misspelled words, a certain letter printed promiscuously amongst others, words of a certain length, different parts of speech, etc.

The quickness of the work was measured by the time taken to do a given amount, or in some cases by the amount done in a given time. The accuracy was measured in two ways, (1) by the number of omissions, and (2) by the proportion of the words

[1] *Corrigenda.*—In Table IV. of the previous article of this series (page 385 of the July number of the REVIEW) there are three errors in computation for which I am responsible. In the 5th column 82.3 should be 92.3; in the 6th, 123.3 should be 123.2; and in the 8th, 40.3 should be 41.3. In the next to the last line of the table 60 becomes 67, and 63, 62.

EDWARD L. THORNDIKE.

marked to those which should have been marked. A comparison of tests before and after training can thus be made. It is wise to use two measures of accuracy, since a change from say 4 to 2 omissions does not mean so much improvement as 50 per cent. On the other hand, a change from marking 96 to marking 98 per cent. of the words that should be, means an improvement of more than 2 per cent., for the more accurate one is at the start the harder it is to gain further accuracy.

The early and late tests were rarely with perfectly equal tasks. If you use the same pages after as before training, you get a probability of practice effect from the first test itself. If you use different pages, they are of course of slightly varying difficulty. Individual records, then, must not be taken too seriously. General tendencies are all that we pretend to demonstrate.

The experiments seem to be fairly good ones, for they concern processes comparable to the training in school life which pretends to improve our general habits and powers of attention, discrimination and accurate work, and are still easily administered and calculated.

We may first examine a sample test in some detail and then recount briefly the results of the others.

Five subjects practiced marking the words containing *e* and *s* in a book containing matter of about uniform style and difficulty and character of topics. Before they began and after they had attained considerable improvement they were tested with marking words containing *i* and *t*, *s* and *p*, *c* and *a*, *e* and *r*, on similar pages (different pages being used before and after training), with marking words containing *a* and *n*, *l* and *o*, and *e* and *r*, on pages different from those used in the training series in length of lines, size of type and style of matter (the same pages being used before and after training), with marking the misspelled words on a page containing a hundred such, with marking the letter *A* on a sheet containing 500 capital letters in a random arrangement.

Their records are given in Tables I., II. and III. Tables I. and II. give the improvement in the training series. There was equality in the length of the first and last pages marked, but

unluckily the first page had an unusually large number of words containing *e* and *s*, making it a harder page. The second page on the other hand had fewer than those which happened to be final pages for the different subjects. Under A we have given the records for time, percentage marked and omissions in the case of the second page, and under B in the case of the first page. Under C we have given the records for the last page in the case of each subject, and under D the records for the average of the last four pages.

The comparison from A to C seems the best to go by. This comparison makes the training seem a little more than it perhaps was, in that the errors of the last trials are more below the average of the last four trials than they should be, but this is offset by the facts that the tests under C had 12 per cent. more *s* and *e* words than did those under A and that the latter were second tests.

TABLE I.

IMPROVEMENT IN TRAINING SERIES.

	Time.				Percentage Marked.				Omissions.			
	First Training.		Last Training.		First Training.		Last Training.		First Training.		Last Training.	
	A.	B.	C.	D.	A.	B.	C.	D.	A.	B.	C.	D.
Ber.	185	235	150	151	78	92	96	93	16	8	4	6
Br.	245	290	120	118	86	100	90	93	10	0	8	5
Be.	240	262	150	155	83	76	81	75	12	28	13	20
Wh.	185	225	105	101	81	90	98	90	13	12	2	7.5
E. M. T.	165	190	105	111	84	83	95	95	11	20	4	4
Totals.	1020	1202	630	636					63	68	31	42.5

Table II. gives the ratio between the end and beginning of the training series in time and accuracy according to the

TABLE II.

RATIOS OF END OF TRAINING SERIES TO BEGINNING.

	Time.	Percentage Marked.	Omissions.
Ber.	81	123	25
Br.	49	105	80
Be.	62.5	98	108
Wh.	56	121	75
E. M. T.	64	113	36
Totals.	62	115	50

A–C comparison. In these and all following tables figures referring to time are given in seconds unless otherwise stated.

TABLE III.

IMPROVEMENT IN TEST SERIES IN TIME.

THE FIRST COLUMN UNDER EACH HEADING REFERS TO A TEST BEFORE TRAINING, THE SECOND COLUMN TO ONE AFTER.

	i-t		*s-p*		*c-a*		*e-r*		*a-s*		*l-o*		*e-r*		Misspelled Words.		A's.		All Tests Together.	
Ber.	200	150	165	160	170	163	210	172	62	68	55	40	65	68	175	160	75	79	1177	1060
Br.	176	146	170	95	140	110	225	203	68	45	64	35	85	63	120	108	110	85	988[2]	795
Be.	232	180	184	170	186	170	306	225	101	71	78	54	108	73	190	150	—	—	1385	1093
Wh.	190	175	170	120	120	140	175	180	80	55	65	45	70	60	135	120	78	75	1083	970
E. M. T.	170	115	146	105	165	120	183	165	85	48	65	46	68	50	140	103	97	85	1119	837
Totals.	968	766	835	650	781	703	1099	945	396	287	327	220	396	314	760	641	360	324	5752	4755
Ratio in training series.																				
Ber. 81	75		97		96		82		110		73		105		91		105		90	
Br. 49	83		56[1]		78		90		66		55		74		90		77		81	
Be. 62.5	78		92		91		73		71		70		68		79				79	
Wh. 56	92		70		117		102		69		70		86		90		96		90	
E. M. T. 64	68		72		73		90		57		71		74		70		88		75	
Totals. 62	79		78		90		86		72		67[1]		79		84		90		83	

[1] Possibly 155 above and so .90 here and a higher average below.

[2] Br. *s-p* test thrown out.

Table III. gives the improvement in time in the various tests. Part *A* gives the absolute quantities and Part *B* the ratios of the tests after training to those before.

TABLE IV.

IMPROVEMENT IN TEST SERIES IN ERRORS.

	i-i		*s-β*		*c-a*		*c-r*		*a-n*		*l-o*		*c-r*		Misspelled Words.		*A's.*		All Tests Together.	
Ber.	17	23	5	2	5	5	38	35	9	13	3	5	3	2	10	9	5	3	95	97
Br.	19	27	4	6	8	11	15	17	14	14	0	4	2	2	6	1	2	1	70	83
Be.	30	16	8	2	12	6	24	22	13	15	1	1	3	5	1	7	17	5	109	89
Wh.	10	5	1	1	10	3	34	17	8	10	2	0	0	0	11	5	1	0	81	41
E. M. T.	38	18	6	2	8	7	49	22	6	12	2	2	3	3	7	10	0	0	119	76
Totals.	114	89	28	13	43	32	160	113	50	64	8	12	11	12	35	32	25	9	474	386
Ratio in training series.																				
Ber.	25		40		100		92		144		167		67		90		60		102	
Br.	80	135	150		137		113		100		∞		100		16		50		119	
Be.	108	142	25		50		92		115		100		167		700		45		89	
Wh.	15	53	20		30		50		125		0		100		45		0		51	
E. M. T.	36	50	33		88		45		200		100		100		143		100		64	
Totals.	50	78		46		74		70		128		150		109		91		36		81

Table IV. gives in a similar manner the improvement as measured by the number of errors.

Table V. gives in a similar manner the improvement as measured by the percentage of words marked.

TABLE V.

IMPROVEMENT IN TEST SERIES; PERCENTAGE OF WORDS MARKED.

	Training Series.	*i-t*	*s-p*	*c-a*	*e-r*	*a-n*	*l-o*	*e-r*	Misspelled Words.	*A*'s.
Ber.	123	92	119	100	104	76	66	104	101	102
Br.	105	87	88	91	98	100	55	100	105	101
Be.	98	127	146	120	102	85	100	91	96	112
Wh.	121	107	125	122	124	88	129	100	107	101
E. M. T.	113	145	127	103	149	70	100	100	97	100
Totals.	115	108	119	107	113	82	89	99	101	103

By testing eight other individuals with three of the tests in the same manner as the five of the tables, save that the former had no training whatever between the tests, we gained an approximate measure of the improvement to be expected on the after-training tests because of their being a second trial. The ratios of their speeds were 95, 91, and 86, but the sum of the omissions of the eight rose from 14 to 20, 8 to 10, and from 36 to 48. So no improvement can be demonstrated in their records.

It is clear from the tables that the improvement in the function of observing and marking words containing *s* and *e* is not equivalent to improvement in the group function of observing the make-up of words. Neither the speed nor the accuracy acquired in the training is a general power equally applicable to other data. And although the functions operative in the tests were so similar to that trained the loss of efficiency with them is considerable.

Speed is more likely to be improved than accuracy. This may mean that certain habits of eye movements and stops are formed that are identical elements in both functions trained and tested. The most notable improvements in accuracy occur with *s-p* and *e-r* on the same style of page as the training series (ratio of percentages of words marked 119 and 113). But here again there are identical elements, observing the letter *s* in the first,

observing the letter *e* in the second, and the width of column, similar sort of distractions, etc., in both. Of the three tests with a different sort of page from that used in the training, *e-r* shows again the most improvement (here the least deterioration) in accuracy.

We cannot ascertain just which of the functions tested improved most and so cannot discuss the way in which greater alteration of the data alters the efficiency of the group function. The reason for this is that we cannot equate time saved with accuracy lost or *vice versa*. As was stated on page 554, the variability of any single test makes any minute examination of the tables unprofitable.

We shall present the experiments of which this is a type in a still more condensed summary.

(*a*) Subject T.

Training: 90 minutes' practice marking verbs; time for first 10 pages 417, errors 10; time in last 10 pages 341, errors 1; percentages 82.8 and 10.

Tests: (1) Other parts of speech; before training 412, 15; after training 398, 4; percentages 96.6, 26.3. (2) Marking words containing *s-p*, *i-t*, etc., before training 638, 8; after training 610, 9; percentage 96, 112.

(*b*) Subject T.

Training: 123 minutes' practice marking prepositions. Time at start 907; errors,—mistakes 16, omissions 10; time at end 756; errors,—mistakes 2, omissions 4; percentages 83.3, 12.5 and 40.

Tests: Other parts of speech; before training 308, 4; after training 314, 3; percentages 102, 75.

(*c*) Subject T.

Training: 90 minutes' with verbs and 30 minutes with prepositions.

Tests: Marking words over 7 letters and words over 5 letters; before training 233, 2; after training 188, 2; percentages 81 and 100.

(*d*) Subject T.

Training: 72 minutes' practice marking adjectives; time at start 620, errors 19 or 30; time at end 488, errors 5; percentages 79 and 26 or 17.

Tests: Conjunctions and pronouns; before training 142, 6; after 127, 2; percentages 90 and 33.

(*e*) Subject T.

Training with prepositions and adjectives as described.

Tests: (1) Marking words containing *s-p*, *i-t*, etc.; before training 700, 9; after training 619, 6; percentages 88 and 67; (2) marking words of 5 and of 7 letters; before training 188, 2; after training 203, 0; percentages 108, 0.

(*f*) Subject T.

Training with words containing *e* for 80 minutes. Time at start 543, errors 5; at end 468 and 5; percentages 86 and 100. Also some 20 minutes training in marking words containing *a* and *n*.

Tests: Words containing *s-p*, *i-t*, etc. Before training 441, 4; after 367, 9; percentages 83 and 225.

(*g*) Subject T.

Training with words containing *e* as above and with words containing *a* and *n* for 188 minutes. Time at start 583, errors 43; at end 436 and 13; percentages 75 and 30.

Tests: (1) Conjunctions and pronouns; before training 136, 3; after 149, 4; percentages 110 and 133. (2) Words of 7 and 5 letters; before training 203, 0; after 185, 6; percentages 91 and —. (3) Words containing *s-p*, *i-t*, etc.; before training 441, 4; after 376, 7; percentages 85 and 175.

(*h*) Subject T.

Training: All the training so far described plus a vast amount of work in correcting all the work recorded so far.

Tests: Marking Latin verbs, prepositions, adverbs, and conjunctions, and French and German verbs; before training 840, 18; after training 764, 11; percentages 91 and 61.

(*i*) Subject W.

Training: About 250 minutes' practice in marking English verbs. Time at start 617, errors 22; time at end 431, errors 11; percentages 70, 50.

Tests: (1) Marking various other parts of speech; before training 42 minutes 36 seconds, errors 15; after training 40 min-

utes 55 seconds, errors 50; percentages 97, 333; (2) marking parts of speech in French texts: before training, verbs 272, 3, adjectives 407, 3, adverbs 440, 10; after training 336, 5; 402, 4; and 394, 8, respectively. Totals before, 1,219, 16; 1,132, 17; percentages 93, 107.

(*j*) Subject W.

Training: That described, plus training in marking words containing *e* and *t*. In the latter the records at start and finish were 981, 20; 569, 23; percentages 58 and 115.

Tests: (1) Words containing certain other combinations of letters. The results were:

	Before Training.		After Training.		Percentages.		Percentages of totals.	
s-p	439	9	503	7	90	82	88.5	105
i-t	827	13	643	11	78	85		
r-e	432	10	271	6	63	60		
l-o	271	2	230	3	85	133		
a-n	412	11	438	20	106	182		

(2) Words of over 6 and of over 7 letters.

Over 7 letters	386	7;	364	2 }	94	40
" 6 "	392	8;	364	4 }		

(3) Marking logarithms containing certain pairs of numbers. The results were:

In Marking.	Before Training. Time.	Errors.	In Marking.	After Training. Time.	Errors.	Percentages.	
4 and 8	118	–	3 and 7	125	–		
0 and 5	130	–	4 and 1	133	–		
	248	2		258	6	104	300

(4) Before training, marking words containing *oa*, *on*, *ti*, *es*, the two letters being in each case together in the order given; after training words containing *ea*, *io*, *ei*, and *ed*, also in each case together and in order. The results were:

	Before. Time.	Errors		After. Time.	Errors.	Percentage of Totals.	
oa	387	0	*ea*	434	8	89	69
on	343	23	*io*	338	1		
ti	580	10	*ei*	358	5		
es	565	2	*ed*	535	10		
	1875	35		1665	24		

231 cases of such combinations should have been marked in the early tests, 167 in the late. Thus, the percentage marked of those that should have been marked was 85 in the early tests, 86 in the late.

(*k*) Subject W.

Training for 27 minutes (all at one sitting) in marking words containing *c* and *t*. Time at start 261, errors 1; at end 134, 11; percentages 51, 1100.

The functions tested before and after this training were the marking of words containing certain other combinations of letters. The total time before training was 655 seconds, and errors 12; after training 482 and 28; percentages 74 and 233.

(*l*) Subject E. T.

Training: About 90 minutes' practice in marking French verbs. Time at start 390, errors 16; at end 353 and 5; percentages 90.5 and 31.

Tests: (1) Words containing *s-p*, etc.; before training 740, 18; after training 666, 14; percentages 90, 80. (2) English verbs, adjectives and conjunctions; before training 557, 23.5; after training 525, 19; percentages 94, 80.

(*m*) Subject E. T.

Training: About 110 minutes' practice in marking words containing *a* and *t*. Time at start 720, errors 15; at end 586 and 8: percentages 81 and 60.

Tests: Words containing *s-p*, etc. Before training 664, 14; after training 577, 21; percentages 87, 150.

(*n*) Subject E. T.

Training: That of both (*l*) and (*m*).

Tests: (1) Words containing *i-t*, etc. Before training 778, 7; after training 516, 33; percentages 68, 471. (2) Words of over 6 and of over 8 letters; before training 282, 4; after training 249, 7; percentages 87, 175.

(*o*) Subject M. T.

Training: About 75 minutes' practice marking words containing *a* and *t*. Time at start 610, errors 127; at end 575, 95; percentages 94, 75. In the early pages there were 218 words

containing *a* and *t*, in the last pages there were 199. The proportions of words marked were thus 42 and 57, the final record being 136 per cent. of the record at the start.

Tests: Marking words containing *i-t*, *r-e*, *l-o*, *a-n*, *g-m*, *a*, *d*, *c*, *h*, six or more letters, eight or more. Before training 1290, 435; after training 1365 or 1305, 456; percentages 106 or 101, 105.

That the tests used in all these experiments were not unfair by reason of being tests of functions that could not be easily improved in any way is shown by the fact that in training series verbs, adjectives, prepositions, words containing *e*, *e* and *s*, *a* and *n*, *a* and *t*, *e* and *t*, all showed ready improvement when special practice was made a factor. W. added to this evidence by taking short training series with words containing *a* and *d*, *n* and *t*.

In about 22 minutes' practice with the former the time remained constant but the errors decreased from 17, 9, 10 and 15 in the first four pages to 4, 2, 6 and 1 in the last. Percentage 25.

In about 17 minutes' practice with *n* and *t* the times decreased from 66, 53, 46 and 64, to 39, 39, 48 and 34 ; the errors from 4, 4, 2 and 3, to 1, 2, 1 and 3 (*i. e.*, from 13 to 7). Percentages 70 and 54.

In the three articles of which this is the last we have endeavored to present as succinctly as possible the results of our experiments. It has not seemed worth while to subject them to a minute analysis, for the reliability of any individual determination is not sufficient to warrant special conclusions from it. The general attitude which comes from the examination of all the facts we have demonstrated, not a set of precisely formulated judgments, is what we have aimed to produce.

The next steps in the study of the interdependence of mental functions would seem to be the exact analysis of the influence of one on the other where such is present and the discovery of its amount and nature in cases of practical importance, for instance, in the case of the training given in school subjects, in occupations, in games, etc. Under the first head we should

hope to see experiments carefully devised, as these rough ones of ours were not, to detect the exact elements of any function that were changed by training, to measure such changes and to find which of such changes brought about increased efficiency in other functions and how. Under the second head we should put determinations of the exact improvement in the efficiency of various functions by commonly practiced educational disciplines and measures of the influence of the training of certain mental functions by school subjects on the efficiency of other functions.

A GENETIC VIEW OF SPACE PERCEPTION.

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In the last half century the theory and general principles of evolution have modified almost every question in philosophy and science. As yet, however, few writers on space perception have given evolutionary principles much prominence in their discussion. In order to obtain a true view of the genesis of space perception it is necessary to ask, "What is the biological value of space perception?" or "What are the biological reasons for the existence of such perception?" This is the question which biologists are now asking regarding organs and instincts and one which psychologists must in the future ask regarding every mental power. Weismann's theory of the non-inheritance of acquired characteristics makes it necessary in answering such questions to show that the organ, act or power does or has helped in the survival of the species possessing it as well as that it might have been developed by use which is all that Spencer seeks to show regarding space perceptions.

Little argument is needed, however, to show that the ability to perceive space relations gives animals possessing it an advantage over those without it in the struggle for existence, for it is evident that any organism that is to survive must have a tendency to so move in response to stimuli as to secure those that are advantageous and avoid those that are destructive. This means that an object in one position shall be discriminated and reacted to differently from the same object in another position; otherwise food would as often be run away from as approached. To react favorably, therefore, there must be a peculiarity in the effects of a stimulus according to the position from which it comes that calls forth an appropriate motor response.

As soon as we take this attitude and look upon space reactions and space ideas as a means of survival we see at once that cer-

tain prominent theories of space perception may be thrown out on the ground of theoretical improbability. For example, James holds that every sensation has the characteristic of voluminousness, but as different responses for different voluminousness of sensation, even if such were experienced, would have almost no biological value as compared with variations in response for different directions and distances the theory may at once be thrown out as improbable.

On the other hand the evolutionary view gives a rational explanation to wide groups of facts, such as that the most mobile portions of all animals, including man, give the finest space discriminations. Evidently sensitiveness to the position of an object can be of no value to an animal except as it calls forth a useful response. Naturally the movable parts, therefore, have the greatest sensitiveness to locality. Other parts are sensitive spatially only because sometimes advantageous response may be made by moving the whole body or sometimes by carrying a movable part to the immovable portion that is being stimulated.

This view of space does not at once justify the hasty conclusions that the muscular sense, and the muscular sense only, is the spatial sense, for movements in any or all directions are of no value to an organism in themselves but only as they help in securing favorable and avoiding unfavorable stimuli. On the other hand the spatial data given by another sense as to the position of a stimulus would be of no value in itself, for it would give no means of appropriate response to the stimulus. Distance and direction have no significance or meaning except as distance and direction from something; hence, tactile spatial data would be of no value except as related to data depending on position of the body. Every spatial reaction that is biologically useful therefore involves the correct relation of (1) a stimulus, (2) the position of the body or one or more of its parts, (3) a movement of the body or a part of it. These are of no value separately but only as related in the reactions. It follows, therefore, that there is no such thing as a single space sense, or sensation for space perception depends upon the relation of at least three groups of sensations or their images.

Given the organic and equilibrium group of sensations and the movement sensations, then a stimulus to any special sense may call forth a response that produces increase or decrease of the stimulus. Such a response would be spatial, and would furnish data to consciousness, if the organism possesses any, for the perception of space. Smell, for example, is in this way to the fox a spatial sense, for he discriminates which of his movements increase and which decrease the odor that he has sniffed, and this guides him towards or away from the odorous object. The primitive spatial data are probably nothing more than the peculiarities of reactions that secure increase of favorable and decrease of unfavorable stimuli when the organic and equilibrium sensations are of a certain kind. Taste, smell and hearing, as well as sight and touch, are spatial to man in this way. Touch and sight are spatial in a much more definite and exact way, not primarily because of the intellectual advantage they give for the formation of abstract ideas of space such as are usually enumerated by psychologists, but because the good of the organism demands that a different response shall be made for each different part of those sense-organs that is stimulated. The only valuable response to smell, taste and sound stimulations is that which leads directly or indirectly to increase or decrease of the sensation, and, in general, only a very few kinds of movement are needed to accomplish these results. In the case of touch and sight, however, each portion of the sense-organs stimulated requires a different motor response. Several different appropriate responses to each stimulus are also possible, such as withdrawing the body or pushing the stimulus away with one of the limbs or bringing it to a position where it may more effectively stimulate another sense, as the eye or the tongue, and thus indirectly secure favorable results. The eye and the skin have therefore developed local characteristics which are associated with correspondingly different movements, and thus sight and touch, in connection with movement, furnish consciousness with most of the vast number of local signs that make definite space perception possible.

That spatial perception is primarily and for all practical purposes (except that of psychologists) merely incidental to use-

ful modes of dealing with objects is clearly indicated by common observation and experimental data. Experiments in moving the hand as directed by the eye show a remarkable degree of accuracy, but, on the other hand, where the attempt is made to *consciously* compare visual and motor perception of distance the errors are very large. The same is true in comparing visual with tactual in one region, as on the tongue, with visual or tactual in another, as on the finger or the back. Even the supposedly non-spatial sense, hearing, is a marvelously accurate spatial guide in practical activities, such as moving the vocal organs or the bow of a violin.

It is clear that, whatever the conscious states of an animal may be, he must, from the first, in order to survive, be a spatially reacting organism. We find, therefore, spatial relations indicated in the structure of all but the very lowest animals, for they have at least an upper and lower surface, and usually a front and hind end and a right and left side. In the lower animals, such as worms, each portion of the body is provided with a ganglion and a sensory and motor nerve. In the higher animals the sensory and motor fibers of the same nerves go in general to the corresponding portions of the body. Parts physiologically connected but not located near each other are connected by means of fibers in the central nervous system, as the front leg of a dog with the opposite hind leg. Human anatomy and psychology clearly show that man also is physiologically a space-reacting organism.

Turning now to the facts observed by students of children we shall find abundant evidence that man is a space-reacting organism *before* he is a space-perceiving intelligence. Four or five months before birth the child begins to move. Much of this motion is, so far as can be determined, spontaneous, being called forth by the internal growth changes rather than by external stimulus. External stimuli, however, such as pressure or temperature stimulations applied to the abdomen of the mother, or gravity and pressure stimulation resulting from changes in the mother's position, do produce movements. Such movements seem to be convulsive-like movements of the whole body rather than localized reflexes, though no very positive statement can be made on this point.

Immediately after birth localized reflexes in response to skin stimulations are numerous. A touch on the inside of the hand causes the clasping movement of the hand, and the toes clasp in a similar way when touched. If the back of the hand is tapped the hand is removed and withdrawal of feet may also be produced by stimulating them. A touch on the lips produces the sucking reflex and a touch on the cheek (especially if there is hunger) a blind groping with the head. A touch on the eyelid, the lashes, or the cornea, and often on the nose or forehead causes the eyes to close.

Active touch, in which sensations are intensified by motion and mingled with muscular sensations, begins in the touching of things with lips and tongue. Almost from the first any object, if not sucked, is mumbled. The fist is most often the object that is brought into contact with the lips and tongue, probably because of an instinctive tendency toward that position. The tendency to clasp with the fingers develops in two or three months into a tendency to scratch or rub the fingers over whatever is touched. The tendency to feel of objects with the hand is clearly evident at the close of the first quarter though for a time less strong than the tendency to feel of them with the lips. In the second and third quarters various parts of the body and objects in various positions are examined by the hand or sometimes other parts of the body, as the feet, though often for a time by the forefinger only, in connection with sight. Movement of hands to parts of the body touched are occasional in the first half year, even at the beginning of the second quarter, and common and accurate at two years. It is evident, therefore, that there is a reflex physiological space relation of stimuli on certain parts of the skin to certain local movements. In some instances these reflex relations are fully developed at birth while in others experience is necessary to their development.

Equilibrium movements toward balancing the head are also evidently reflex and instinctive almost from the first, while movements toward straightening the body when tipped may appear as early as one or two months. In the case of the *eyes* there is from the first a reflex tendency to turn the point of clearest vision on an object seen. This is shown in the tend-

ency to turn the eyes towards a light, and sometimes towards an object at one side, and to keep the eyes directed toward any object in direct vision even when that object moves slowly. This reflex tendency develops into sure and accurate turning toward and following of not too rapidly moving objects in the first quarter.

Closing the lids at the *visual* stimulus of a threatened blow is an undeveloped reflex, for it does not take place regularly until two or three months, though the visual stimulus helps to insure the closing of the lid at a tactual stimulus near the eye much earlier.

The visual stimulus of objects at a distance exercises much the same influence upon the hand movements as tactile stimulation on different parts of the body, for from the first attempts at reaching for objects in the second quarter of the first year children habitually reach in the right direction and for near objects only. The latter statement is contrary to common belief and the statements of psychologists, but I know no reliable evidence to the contrary. The supposed instances of children reaching for distant objects are, I believe, the products of imperfect observation and interpretation of children's movements, for the instinctive tendency to lean and reach toward any object of interest may be easily mistaken for an attempt to grasp the object. Baldwin emphasizes the dynamogenic effect of near over far objects as a physiological fact of great importance and all his records of reaching for colored papers show great accuracy in judgment of distances by his children, yet he indorses, without giving a fact in support of it, the old idea that a child will reach for the moon, meaning by the moon an object at any distance. I find no facts recorded by observers of children that, properly interpreted, support this old-time and almost universal opinion. Preyer mentions two instances, neither of which, properly understood, counts against the view of natively differing dynamogenic effects of objects at different distances. In one case a child holds an object out of a second story window for an adult on the ground to take. This merely show's the child's faith in the reaching ability of the adult and is no evidence of a misjudgment as to his *own* powers of reach-

ing, for he did not attempt to *hand* the object to the one below. It shows lack of knowledge of general spatial relations but not of space as related to his own practical activities. In the other instance a boy reached for a light in a car a number of times, laughing hilariously in the meanwhile. Clearly he was not engaged in disappointing and unsuccessful attempts or he would not have laughed with such glee. He was merely playing and enjoying the movement and perhaps the make-believe attempts at reaching. Apparently it was the father, the psychologist, rather than the child, the actor, who was deceived.

In the case of my little girl close observation for several months after reaching began revealed no instance of her attempting to grasp an object more than six inches beyond her reach. In the case of my little boy the expressive movement of holding out the hand toward an interesting object was not so easily distinguished from attempts at grasping, but there was clearly no grasping at objects beyond two feet. If an object was more than a foot away he leaned toward it as well as reached, while if it was six inches he reached with his hands only. In both children I observed instances in which they did not seem to know whether they had reached too far or not far enough when the error amounted to an inch or two. This shows a judgment of distance only a little less accurate than the judgment of direction, for objects were sometimes missed, especially when they were above or at one side of the face. I am convinced that the so-called judgment of direction and distance is not primarily conscious but physiological, the objects in the various positions constituting a different stimulus and calling forth a correspondingly different movement. The case is similar to that of the young chicken, which, from the first, picks at objects with considerable accuracy. The physiological tendency is the same, though not developed so perfectly at birth in the child as in the chicken.

Reactions to *taste* stimulations are from the first spatial, in that disagreeable tastes produce movements of getting the substance out of the mouth while agreeable ones cause movements toward increase of sensation and swallowing. Mucus is frequently ejected, and after hunger is fully satisfied the nipple

is often forcibly ejected by the tongue. Movements of withdrawal in response to disagreeable odors may be called forth quite early, but sniffing to inhale an agreeable odor does not appear until in the second year.

As to hearing, the instinctive tendency to move so as to increase favorable stimuli is specialized to some extent into a reflex tendency to keep the head turned directly toward an interesting sound which, at the close of the first quarter, develops into a tendency to turn toward any sound with accuracy, though the effect of experience is marked, for familiar sounds are more readily located than new ones.

Our study thus far shows that the child is at birth a spatially reacting organism, and since most of the movements during the first few months are made in sleep as readily as when awake and are made by children born without brains as well as by normal children we conclude that consciousness has at first nothing to do with the making of these movements. They are therefore purely physiological. The more complex instinctive and voluntary movements that appear a little later are the result of the development of semi-native reflexes and instinctive movements and of various combinations of them, the tendency to which is also native; hence they also are more physiological than psychical. Consciousness at first can merely cognize such movements. It cannot possibly represent and control a movement until after the movement has been made one or more times. Even in the case of adults movements often repeated and practically directed are not necessarily consciously known. For example, a large proportion of bicycle riders cannot and never could tell which way they turn the handle bars when a bicycle tips to the right. (My own students were divided almost equally on this question.) One part of the body may also have learned to adapt its movement to a visual stimulus when another has not, as has been proven by Reddingius. He found that when one hand has been trained to accurately touch a point seen through a deflecting prism the other hand still made an error proportional to the apparent displacement, while when the prism was removed the trained hand made the opposite error.

The spatial data given consciousness is from the first com-

plex and relational and always dependent upon position of body, direction and distance of stimulating object and movements of body, head, eyes, or limbs. The result of a reaction is the important thing biologically and the interesting thing psychologically rather than the sensations experienced while making the movement; hence little attention is given to the spatial data. Since adults who have not given special study to the subject know almost nothing about the elementary spatial data given, and since children have an almost infinitely less power and tendency to subjective analysis than adults, it is utterly preposterous to suppose that they are at first conscious of the elementary data of space perception as is often imagined by philosophic students of space perception.

The analysis of spatial data by the child probably begins in the practical need of distinguishing between its own body and other things, though the instinct of curiosity has some influence. The marked difference in the sensation complex which the child feels when he treats his own members as he treats other objects leads him to notice the distinction between his own body and surrounding things. At three or four months a child often shows that it has made this distinction by manifesting disappointment and impatience when it brings an object toward its mouth and gets its own finger instead of the object in contact with the lips. This early discrimination of the body from other objects is very crude and only along lines of familiar experience, for a child of a year or more may intentionally strike his hand or foot as he has been striking something else, to his immediate surprise and grief. Even in the third year a child may ask to have his fingers taken off with no more thought of the result than when he asks to have his shoes removed.

Biologically the most important portion of space is evidently in the region of the mouth and there is every reason to believe from a study of children that the first conciousness of space is of relation to the mouth. The first movements are to increase and secure gustatory and tactile sensations, and the tongue and lips are for some time the chief organs of active touch. Auditory, visual and olfactory perception become clearest when the mouth turns toward and approaches the stimulating object.

The hands are from the first often in the mouth, they move a great deal in that region and they continually go out from it to bring objects to that center of the child universe. Voluntary control is first established and always remains most perfect in front of the mouth, while movements high up, far down, or to one side are much more difficult to make with accuracy. It is not too much to say, therefore, that spatial perception first appears in the perception of the relation of other parts of the body and of objects to the mouth. In general this region also remains the most favorable one for conscious judging amounts of space as well as for making movements.

As to the further problem of space perception involved in the discrimination of distance, size and form, the principles involved may best be studied in connection with visual perception. In considering this problem we must take pains to avoid the common fallacy of supposing that a perception of a whole means the perception of the parts and their relations or that the perception of the meaning of complex sensory data involves a knowledge and consciousness of the elements of such a complex. We must also remember that a child has far less tendency to note and analyze his subjective sensations than adults. The psychologist is very much interested in all the spatial data given in the child's sensations, but the child himself cares nothing about them, his consciousness being absorbed in trying to do something with objects in various positions. Not space, but the recognition and manipulation of objects, whatever their position, are the interests directing his movements and thoughts.

As regards distance we have already seen that the dynamogenic effect of an object at a distance and one near by is such that, from the first attempt at grasping, the latter is reached for while the former is not. Judgments of size have been supposed to depend upon the amount of retinal surface covered by the image of an object; hence it is inferred that objects at a distance look small to children. Experiment, however, shows that for adults artificial increase or decrease in size of image on the retina may affect the judgment of distance instead of size and that change in ocular adjustment may affect judgment of size instead of distance. A slight change in attention or the introduction of other

objects in the field of view readily reverses the judgment of size and distance. Usually the individual does not know the basis of his judgment or why he changes it. The judgments of size and distance, therefore, depend upon the same complex data and are relative to each other, so that as soon as one judgment is passed according to the ideas suggested by the complex of sensations and images, the other follows automatically.

The child cares nothing about size and distance as such, but wishes merely to identify and manipulate objects. He knows nothing about size and distance apart from the other, to him, as yet unanalyzed characteristics of objects as wholes; hence he does not think as some have supposed that objects grow small at a distance, though he does often have difficulty in recognizing objects seen at unusual distances, just as an adult would probably fail to recognize near at hand a familar figure that had been seen only on a church steeple. He is able to compare and identify objects at varying distances just as we can judge of the equality or difference in size of rectangles of various shapes without knowing either of their dimensions. Such comparison is easy for him just in proportion as the clearest and most familiar appearance is suggested by the appearance at the distance from which it is being viewed.

At first, and for some time with most objects, the child probably does not think of objects as being large or small, distant or near, but merely identifies interesting and often-perceived objects, whether near or far, and recognizes that he can often perceive an object better by moving toward it. As long as an object varies constantly in appearance as it or the child moves nearer or farther, there is no occasion to analyze out from the complex whole the characteristics of size and distance. When, however, an object familiar within certain distances is seen *much* nearer or farther away than ever before, it is either not recognized or attention is directed to the change in appearance and this may lead to the analysis of the spatial characteristics of objects from their more unchanging characteristics. This analysis is, however, chiefly brought about and made exact by comparing objects when they can be perceived most plainly, *i. e.*, when near by and directly in front, and where touch sensations also

aid in the perception. The spatial appearance of a familiar object when most clearly seen, therefore, comes to be accepted as its true appearance or size, while variations from this standard are connected with movements and interpreted as distance.

As to form and solidity it is perfectly true that every line, angle, surface and solid, except the sphere, affects the retina differently for every different direction from which it is seen, but untrained adults do not know this and sometimes cannot be made to believe that a square always looks like an oblong or a rhombus, when not seen at right angles to the line of sight. It is utterly improbable, therefore, that the child, who has so much less power of introspective analysis and who, to a greater extent than the adult, is interested in discriminating and identifying objects, rather than in noting their varying appearance, is conscious of the presence or absence of solidity or of apparent variations in form. Just as we can judge the relative size of objects of various shapes he can recognize solids as the same in spite of their changing appearances. The differing appearances are enough like the one familiar to him to suggest it and thus enable him to recognize the object.

Within a half year the child is able to identify a number of objects from any position and rarely mistakes a surface for a solid. At two or three years he discriminates hundreds of forms in nature and may distinguish the principal geometrical forms and some letters.

The discrimination of form from other characteristics and its analysis into lines, direction, surfaces and solidity is brought about by the fact that the mode of dealing with objects necessarily varies with their form, and by the comparison of one object with another, especially when they are beside each other in a favorable position for seeing and touching them. The appearance deemed the true one is that of the object when most clearly perceived (rather than any other or a complex of several such distinct views). Several perceptions, as solids are seen and touched when directly in front and turned in various ways, are fused into an idea of the object as a whole. For this reason a child in his early drawings shows invisible as well as visible portions of a solid and often fails to distinguish between a square

and a cube, or a circle and a sphere, both having to him the same essential elements. The following summary indicates some points in the study of the genesis of space ideas that have not in my judgment been sufficiently recognized and emphasized by writers on space perception.

1. Space perception in the child and the race is the result of, and an aid in, useful reactions to surroundings.

2. There is no such thing as a space sense or a sense of space, for space perception is the cognition of the *relations* of sensation groups to each other as reactions are made in the attainment of practical ends.

3. The most primitive space reactions are those that result in increasing favorable or decreasing unfavorable stimuli, and distance is not at first distinguished from other causes of varying clearness of perception.

4. At and probably before birth the child is a space-reacting organism.

5. The chief center of location and reference is in the region of the mouth.

6. Conciousness of space relations is the result of space reactions, not the cause.

7. The child has no interest in spatial data as such, but learns to recognize and manipulate objects in spite of their varying appearance with changes in direction and distance.

8. The clearest and most common appearance of an object gives him his idea of it and other appearances simply suggest this idea.

9. Ideas of direction, size, distance and shape are gradually formed as the result of the manipulation of objects and the comparison of one with another, while the concept of space as usually discussed is an abstraction from numerous experiences and more or less conscious analysis and synthesis.

A CASE OF PSYCHICAL CAUSATION?

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The aim of this paper is to show that there is possibly such a thing as purely psychical causation. We shall begin by trying to apprehend the arguments—which are at once so widely accepted and so weighty—on the other side. Psychical causation is denied by present orthodox psychology for several reasons. (1) Historically, the first to be urged was that there is no necessary connection to be observed between any two states. Hume[1] used this argument to disprove causation in outer and inner worlds alike, but more recent thinkers (following Kant) have tried to restore it in the outer world. It is generally agreed, however, that even there it is only the way of viewing facts (uniform sequences) which our categorizing minds adopt —a subjective construction.[2] Now, while the character of the outer world is such as to allow this subjective construction, that of the inner, we are told, will not permit it. Psychical states lack two attributes which are requisites of causal connection; (2) they are not susceptible of measurement, and (3) they are not continuous with one another. A mental state is not a whole summed of parts, but, being a *consciousness* of something, is indissoluble and unitary, even though its object be divisible.[3] Nor could one conscious state ever merge continuously into another any more than one act of will can merge into another; each awareness is eternally 'shut in its own skin, windowless.'[4] It comes into existence, and it vanishes and is gone forever.[5] So, on the one hand, we cannot find a quantitative equivalence

[1] Treatise, Part III., Secs. II., III., XIV.

[2] Typical of this attitude is Mr. Pearson's 'Grammar of Science.'

[3] James, 'Psychology,' Vol 1, pp. 158–162, and Münsterberg, *Grundzüge der Psychologie*, Bd. I., pp. 263–270.

[4] James, *ibid.*, p. 160.

[5] Cf. Münsterberg, 'Psychology and Life,' p. 59.

between antecedent and consequent psychical states, and, on the other hand, we cannot have that continuous change which is a property of physical causation. Hence, there can be no psychical causal laws. And the argument is clinched by the principle of parsimony. (4) Since causation and necessary connection are the figment of our minds, and not the result of empirical observation, we must not apply them in regions where explanation is simpler without them. And, as the order and character of psychical states can be well explained on a physiological basis,[1] it must be useless to assert psychical causality, even if psychical states were both continuous and quantitative.

Such a weight of reasons indeed appears overwhelming. We are not allowed to regard causation (or even necessary connection) as a property of events themselves (whether physical or psychical), and we are not allowed even to *interpret* causally in the psychical world. But since these reasons are for the most part deliverances not of empirical observation, but rather of a certain amount of analysis, we may at least be permitted a little argument. Take reason (2) above, the first attribute in which the inner world was found wanting. Suppose we admit that psychical states cannot *ever* be measured; what then? Well, we may be obliged to forego the kind of causal laws that we find in the quantitative physical world, but not on that account necessary connection. There is, *a priori*, no need that whatever is necessary be quantitative—even though the converse be true. It is at least conceivable that some event might necessitate some other without their being measurable. Here one need only point to projective geometry and the algebra of logic—both regions of strict necessity yet non-quantitative.[2] Now as to the second defect of mental states—reason (3) above—the argument against their continuity surely confuses psychical states in general with consciousness. These two are always found together, I suppose; but for all that one is not the other. I may be conscious of a toothache, but the psychical state called pain is not the same as my con-

[1] See James, 'Psychology,' ch. on 'Habit' and on 'Association.'

[2] And Wundt himself advocates such a psychical causality (though only as a conceptual description), 'Outlines' (tr. Judd), pp. 320, 321.

sciousness of the toothache—it is the *object* of my consciousness. And no doubt the argument against continuity between *consciousness* of this and *consciousness* of that psychical content is true; but still the psychical contents *themselves* may be continuous. And certainly this latter seems to be, very often, an empirical fact for introspection—one's perception of a moving object (sometimes at least) seems a continuous psychical performance. Indeed, it is one of the emphatic assertions of present psychology that mental states present a steady onward flow.[1]

Now, if this argument is sound, the situation (as far as *facts* go) is not fatally different from that in the outer world. We seem to find gradual change from one psychical content to another, and even though there is possible no equation expressing the identity of antecedent and consequent, still we do not as yet find anything absolutely hostile to non-quantitative causation. But here the law of parsimony, taking its stand on Hume's criticism, confronts us. We can at most only show that there is a regular sequence between psychical states, just as there is in the physical world. The causation, the necessity of the sequence, would be our own convenient formulation of the matter, and as such must be subject to the law of parsimony. We must still forbid psychical causation because we can formulate the facts without it. Even though the idea of two plus two were always followed by the idea of four, we should not say the former caused the latter, but explain it by the physiological basis of association. The only possible ground for our granting this doctrine a place would be that the necessity is not our concept merely, but a real fact external to our categorizing mind. It must be shown that one mental state does of itself, on account of some quality which is part of its own content, necessitate a certain other. It must not merely be followed by that other, for then the sequence *could* be explained on physiological grounds. It must *produce* the other, in such a way that we can *see* the necessity of the connection between them. Could such be discovered as a characteristic of the mental

[1] Cf. Dr. Ward, Art. 'Psychology,' in 'Encycl. Britannica,' and James, 'Psychology,' Vol. I., pp. 237, 271.

states *in themselves*, then the law of parsimony would have no sway over us here, because the causality would be *not our mode of viewing* psychical events—that is, not our ideal or conceptual construction—but the *existing connection* between the facts *themselves*. So it is incumbent here to demonstrate an objective (*i. e.*, not constructed by us) necessary connection between some of our mental states, such as Hume could not find. In other words, Mr. Karl Pearson's doctrine of the subjectivity of causation must be refuted by the pointing out of a case in which necessary connection is an objective fact, a characteristic belonging to the psychical contents of some of our states of mind. To do this, we must first frame a definition of what objective necessary connection would be, and second, inquire if that definition is ever, in the psychical world, actually fulfilled. Thus in proving psychical causation one is practically compelled to prove that there may exist an *objective* necessary connection.

The thing to be shown is, therefore, that there is some one mental state which, if entertained, brings up other mental states, and brings them up in such a way that a real necessity is seen for their appearance. Now, the mental state of which I propose to illustrate this attribute, is the idea of *more* or *increasing* (defined as I shall soon indicate), and the mental states which follow it are the ideas used in elementary arithmetic. These latter must be proved to have such a psychological nature that their presence and their character can be seen to be derived from, necessarily connected with, the original state. The method will be to examine the contents of our mind when we entertain the idea of *more* and to show that they would naturally lead us, of themselves, to entertain certain other ideas; that even though this process seldom goes very far in us, it is because interests interfere, associations distract, or outer stimuli withdraw our attention. Then we shall ask, Does this succession of ideas exhibit that necessary connection which is requisite for true causation? Is the series any more than a uniform sequence, based, say, on habit, on early training? To answer that, we must see what conditions ought to be fulfilled if there is to be necessary connection between original and later ideas. We must designate the marks of necessary con-

nection and inquire if this case has those marks. This part of the task will be a purely logical one. We shall find that the idea of *more* is unique among ideas, in that it has a very remarkable psychological property enabling it to satisfy these logical conditions. And further, we shall find that the ideas succeeding have the same property of producing other ideas, that *more* had. The remainder of the paper will then fall naturally into three divisions. (1) Statement of the psychological contents of the idea of *more* as we shall use it, and evidence that the idea, if entertained, actually induces us, however feebly, to entertain certain farther ideas; (2) statement of the conditions required for necessary connection between antecedent and consequent, and proof that these conditions are fulfilled; (3) detailed account of the series of ideas resulting in the process (the elementary arithmetical ideas) as evincing continually the same productiveness found in the first idea. The interest of this third part lies in this, that we are shown the genesis and meaning of ideas of Number and Quantity, and that arithmetical necessity is proved to be, at bottom, a case of psychical causation.

1. First, then, what is the content of the idea as we intend to use it? That is, what is actually present to consciousness when this idea is entertained? The content is twofold, and consists of the idea of size and movement or change. (Inasmuch as change often means decrease while movement suggests, a little more strongly, an addition of something new, a going on to something new, the latter term will be used.) To see an increasing thing is to see a *size* that grows into a bigger size. Size is here not defined further than by some such equivocal word as bigness. It is what is in the focus of consciousness when we are thinking of the quality *size*—and that is about all we can say. It has no detailed meaning or definition as here used, any more than 'blue' has in "The sky is blue." It does not mean size of a certain amount, and does not imply measurement. And movement is here to be understood as belonging to, or a quality of, the size. These are not two ideas placed in juxtaposition, but organically united—as in a thing of qualities. The thing is the size and its characteristic is movement, taking

on of something new. Its complete description is given, as always with *things*, by one word, *moreness*. This may or may not be a correct description of what is always in our minds when we use the word; but it is an idea which we sometimes entertain, or which we might entertain easily enough, and the names *more* and *increase* come as near to it as any other names; therefore, let us agree to use these words.

Now we have to show that a state of mind, such as this, would naturally lead us to entertain certain further ideas. Let us see what would happen when we saw something that gave us the idea of increase. Suppose I am sitting at the car-window in a railroad station and the train starts and moves out past the switches, signals and bridges. Then, if I happen to reflect on what I see, I say, "The distance from the station *increases*," and there are in my mind the two elements mentioned above. At the first switch I am conscious that we are out of the station, at the second that we are farther out—the distance out (marked by the train's position) is moving on. But let us see what has happened in my mind while I was thinking of the element of movement. I could not have perceived the movement had not the perception of movement been present to my mind *successively*. As long as anything moves, there is a continuance of its motion. If a ball flies a hundred yards through the air, the motion it had during the first ten yards was kept up, was repeated for another ten yards, then another, and so on. And the same repetition is found in our *perception* of motion. If I see a fly on the window-pane, in a certain spot, then suddenly find that he has crawled to another spot without my seeing any intervening changes—without my having a *series of perceptions*—I do not say I *saw* him move; I can, at most, only *infer* a motion. In order actually to see him moving, I must have seen enough of the intervening changes to make the whole process seem a continuous one. I must have seen the process of change *repeatedly*. So with the perception of increasing distance from the car-window. If I am aware of the movement from the station out past a couple of switches to the first bridge, I must have been aware of the movement (however dimly) in each stage of that advance. As

we went from the first switch to the second, it was present; as we went from the second switch under a bridge, this idea of movement came again into my mind. And if at these various stages I had not been aware of the movement, I should not have been able to see the *continued* change, which is a necessary attribute of perceived motion.

This, of course, does not so far show that I have been aware of the repetition[1] as such. The idea of movement does not necessarily induce us to become aware of its own repetitive nature. The idea of *more*, however, *does* induce this awareness, for *more* and *increase* are relative words. Whereas, the movement of the train past several bridges need not force upon me the thought, "Going past bridges is a process often repeated just now," yet the idea of increase in distance tends to suggest the thought of the present distance as contrasted with the former distance. And if movement is a continuous process, certainly increase (as we have defined the word) is equally continuous. Therefore we have (whether aware of our own state of mind or not) the idea of increase present to us repeatedly. But if, each time the idea is entertained, it tends to bring up the contrast between the present distance and the former, there is certainly a *tendency* to get before the mind the idea of a series of increments. It is because the idea of *more* includes *both* the element of movement and that of relation between present and past, that it not only has repeated itself several times, but tends to make us aware of that repetition. To have the idea of something that increases, then, is to tend toward consciousness of a series of increments.

To be sure, this can hardly be proved by introspection—99 times out of 100 the passing bridges or signals lead to no thought of a series of increments in distance. But I think it would be generally agreed that, in these 99 cases, the direction of our train of ideas could be adequately explained by association, interest, or outer stimulus. The difficulty is not to explain why deviations from the above marked-out line occur,

[1] In using 'repetition' here and later, I do not mean to imply that the same individual idea can recur. It is simply (as explained later) another idea with the same character.

but why there should be a tendency along that line. And since we are dealing here with tendencies—and tendencies which, confronted by so many and so strong practical interests in our busy life, must be for the greater part submerged and thrust below the threshold of consciousness, we can hardly expect any proof further than general analogies and probable reasoning. I can only point out that, if our state of mind is such and such, it *would probably* urge our thoughts thus and so. But even this kind of argument is not without some weight. When we consider, for example, that a long-repeated series of repetitions—of rapidly passing telegraph poles—often forces itself on our notice by its very painfulness, and makes us clearly aware of a series of increments—it seems as though the same tendency must be present, even when the time is very short—though below the threshold of consciousness.

The next point to be shown is this: When the idea of an increment in distance tends to suggest the idea of a series of increments, each more than the last, we have a state of mind which itself evinces (if we analyze it) a tendency to widen and bring up still further ideas. This will certainly not be plain to introspection. We cannot immediately see that the thought of a series of *mores* would tend of itself to suggest anything further. But a little analysis can inform us of what is beyond sight. What, then, have we good reason to suppose is really going on in our mental machinery? Here are two (or more) increments, and we are aware that they are related (consciousness of *more* tells us this, because *more* is a relative word). Now what is the nature of this relation—I mean, what is its real psychological nature, irrespective of our being *aware* of that nature? First, it is such that it presents us with a series of terms—that is, a consciousness of terms arranged side by side; in other words, the terms are so arrayed before the mind that the relations existing between them (whatever they may be) could most easily be detected—provided we should happen to look for them. When we want to find the character of the relations existing between any two things, the first requisite is to get them arrayed together before the mind. So here, the materials are ready for the discovery of the relation's character. This, then, is the

psychological character of our consciousness of *more*—that the terms of the relation are offered in contrast, side by side before us. But to effect the discovery of the relation's character, another factor is needed. Unless we were *urged to look* for some kind of relationship, the being arrayed side by side before the mind would have no influence upon our minds. One can very well have the idea of a series of pendulum-beats and yet not tend to ask himself what is the relation between those beats. But if he has the idea *that they are more and more* (as in our case is presupposed) he has a suggestion of a relationship between them. This suggested relationship, now, amounts to a consciousness *that a relationship exists*. But, he does not know the *character* of this relation. For, the character it has (as we have seen) is that it presents the terms arrayed side by side for his inspection of what the nature of their relation is. The character is that the character is waiting to be discovered. The situation, then, is just this: We are made aware, by the word *more*, that there is a relation, but we are not told what sort of a relation it is, though the materials for the discovery are presented. Now, in such a case, what would be the natural tendency of our minds? Why, to try to discover the character of the relation. This tendency is an instance of a universal human attribute, as common in the child's as in the savage's mind—the primitive instinct of curiosity. It is this which makes us turn the eyes in the direction of a light that suddenly appears in the dark, or look around when a blow is dealt us from behind. If a man stands peering into the darkness, and some one says to him, "There is something over there," he will strain eye and ear alike to discover what that something is. An event occurs that acts on our mind as a pointer; the discovery of an *existence* urges the discovery of some *character* in that existence. So when we learn that a relation exists between the elements in front of our minds we naturally look to see what that relation is. Thus we tend to ask, what is the character of the relation between these increments?

We must get this process clearly in mind, for two reasons. The first is that upon it the whole advance depends. Every time we get a new idea, as we shall pass from stage to stage, it will be in this way. We shall be aware of a relation (sug-

gested by *more*) whose character we cannot yet tell, and this will induce us to get a description for it. This description or characterization will constitute a new idea. Each new idea, however, will be found to be an insufficient characterization of the original *more* (for reasons which will appear), and thus we shall find ourselves continually left with an existing, yet still to be characterized relation.

And, secondly, it is indispensable to understand this process, because (I hope later to prove) *such a one alone* displays that necessary connection demanded by causation between antecedent and consequent ideas. For even now, before we come to the definition of necessary connection, we can see that the psychological properties of this process are somewhat unusual. The original *more* led us to repeat itself—or rather (since we were actually repeating it all the time) gave us the idea of the self-same original over again, viewed as a relation. This relation, in asking us to characterize it, is to be restated in a new aspect—and so on. And if this restatement brings in no new materials from without it will be a *repetition*. The discovery of a relation which we characterize would thus lead to a process which is nothing more nor less than a process of *self-repetition*. There would then be something about the original idea that gives us new material, in a sense, and yet that new material, being a *repetition*, derives all its character and properties from its parent. Already it seems like a case of genesis, of actual creation and inheritance of family traits. But until we have seen more of its workings we cannot judge accurately of the process.

It is necessary, then, to go through two or three of the next stages before we can apply completely to the process the definition of necessary connection. So we take them in order:

1st Idea. *Sameness.*—Given the question: What is the character of the relation *more* between the increments? How describe moreness? What can we call it? And we are thereby urged to give it a *name*. And this name is to describe that positive relation (I say positive to signify that we are seeking a definite character) existing between the increments. We want a name for 'the relation between them, viewed as expressing

their nature relative to each other.' This is, whether we know it or not, equivalent to the relation between their contents. (For, the positive relation between any two things means the contrast or relation between their characters or contents.) We want, then, to give some name to the relation between the contents of the several increments. This is done by the word *sameness*. Sameness[1] here means only 'that relation which holds between increments in a series of *mores*.' And let it not be said that some new meaning, not originally present, has come in. For sameness is simply a short word for the cumbrous phase 'that relation which . . . etc.,'—in other words the meaning of sameness is defined solely by reference to our original material. But even though no new material has appeared, we feel that we have really made a discovery, simply because we have coined a name. For it is a human failing, that we regard anything as of more weight when we have dignified it with a name of its own.

2d Idea. *Difference*.—Sameness does not satisfy the demand for description. For it is a psychological fact that when we are thinking "Here is the same thing again," we are (whether conscious thereof or not) tending to forget about the relation and to think only of the content. When we identify or recognize any object, it is necessary to attend to its qualities or contents—for in practice we distinguish things by their qualities. Thinking of the sameness thus leads to the thought of bare self-identity of the content (*i. e.*, as being a *size*) through the series, and here the relation disappears from the mind. So we have on the one hand the relation *more* before us, asking: What is my name? And on the other we have the idea *sameness* which, even while answering the question, leads the mind away from the relative nature of *more* to the characters of the individual members. Now, since we are seeking not so much for the individual characters in the series as for the relation between them, we are still urged to seek the nature of this relation. So we return to a situation much like our first one. Our question now is: What is the name for the *relation* between the

[1] Of course this use of the word is not the only one. The word simply seems the most convenient to designate what is described above.

members (not the contrast between characters, since that did not happen to be fruitful for the discovery of a relationship)? And this name is *difference*. *Difference* means simply the relation holding, not between the individual contents in the series, but between the members as members, *i. e.*, it means a relation in general between them. And no new meaning is here used, for this definition makes use of the original terms only. But again, having gotten a word for the above circumlocution, we think we have discovered a new idea.

So much for the process by which ideas are generated. Of course it is not meant that the idea of sameness, or of difference, first arose in this way. They may have arisen in any way we please: but it *is* claimed that, given the idea of more as above defined, *then* there is a tendency to call up ideas just like those we call by the names sameness and difference (that is, if they have been correctly understood). Nor is it claimed that this tendency is discoverable by introspection (though sometimes it may be). The argument asserts that, on general analogies, there would probably be a tendency to entertain further ideas, if we were aware of an existent something (in this case an existent relation), but were ignorant of its character. In other words, we have general psychical characteristics which give some warrant for the probability that there is a certain idea which tends to generate certain others. Though this is not saying very much, it is still an interesting fact—and it would be still more interesting if this apparent genesis were found to fulfil all the requirements for being an effect-necessitated-by-a-cause. We go now to that part of the inquiry.

2. We must first state the logical marks of necessary connection between events (whether physical or psychical). I mean connection between events themselves, and not merely ascribed to them by our categorizing mind. That is, we must state such logical marks in both antecedent and consequent as would show a real derivation of the latter from the former. Now, to have necessary connection between any two events,[1]

[1] I do not go into any detailed examination of the concept of causation, but discuss necessary connection only; first, for lack of space, second, because I think it would be generally admitted that where one psychical event led of necessity to another, the case would be for all practical purposes one of real causation.

three conditions must be satisfied. (*a*) The character of the second must in the last analysis be the *same* as that of the first. Otherwise there will be something in the consequent which cannot be shown to have sprung from the antecedent. There will be something different in kind in the consequent. This attribute of necessary connection is recognized when physical science equates the energy in cause and effect. But the fact that it cannot point out a complete *qualitative* identity between cause and effect—for instance, between the phenomenon of radiation of the sun's heat and that of evaporation, which appear to us as events of very different natures—has been a strong reason for many men denying objective causation. At least if such perfect identity could be traced from cause to effect—while yet there was some real change of events—one great objection to necessary connection would vanish. (*b*) This last parenthetic clause suggests the second condition: There must be a change of some kind. There can be no necessary connection without two events to be connected. Even though the consequent does not differ in character from the antecedent, it must somehow be a *distinct event*.

This could happen as follows: If I had an idea, no matter of what, and that idea tended to make me keep attending to it—that is, tended to *repeat itself*. Then the repetition would not differ in content at all (that is, in character) though it would involve a change in my state of mind—supposing that I was aware of the second case as other than the first. This second condition need not conflict with the first, then, if I have an idea that leads me to continue attending to it. A *self-repeating* idea therefore satisfies the first two conditions. But the phrase '*leads* me to continue' was just used: now, how do we know that it *leads* me? Do we not assume the necessity that constrains me to continue attending?

This leads us to the third and crucial condition. (*c*) There must be a movement, or process of continuous flow, in the antecedent, such that it gradually becomes the consequent. Even while we entertain the antecedent, we must be conscious that it has a movement in it. This *connects* it with the consequent and enables us at the same time to account for the latter's char-

acter and existence. Its character is what it is, because it is simply *a later instance of* the antecedent (the antecedent moved on)—and it exists because *there was a movement* to begin with (*i. e.*, a change: the first instance changed into the second). Given the *character* of the antecedent and the *fact of its movement*, then we can see that the consequent had to appear, and, if no new elements are displayed in it, that its character is perfectly accounted for. Now, if we could ever find a pure case of this kind, we should be justified in saying that there was something about the antecedent that *necessitated* the consequent. Of course we could conceive the movement suddenly stopping, before the latter was well before the mind; but that would only be an *interruption* of the necessity, which if left alone would still have produced the results. The necessity itself appears in the fact of movement; for movement (presupposed to be continuous), where no foreign materials are brought in, implies so close a connection between its earlier and later stages that the later are offered as *growing out of* the former. And they *have* to grow out because there is begged the attribute of movement. And it does not matter whether there is real continuity or not, between the earlier and later cases—so long as there is apparent continuity; for we are trying to find a sequence which would, as it was present to our consciousness, fulfil the conditions of necessary connection. Our question now is: Can we ever find an event which has character and at the same time a process of change going on in this character, which leads to a series of repetitions of the original?

Such a case is, of course, the one we have been discussing, the idea of *more*. It was defined as involving a size (this was its character) that moved on (this was its property of increase); and this increase tended to make us aware of a repetition of the original. Then this repetition (or series) suggested a new name for itself (sameness as a new name for the original *more*), and after that the discrepancy between sameness and what was required by *more* suggested another name (difference): and throughout we saw this was a process of self-repetition. For sameness we defined wholly by the original ideas, no new idea having appeared; difference likewise was simply a new

name for the old mental content. The first peculiar quality in *more* is that it suggests (by its own *relative* nature) that it is a repetition of itself. And here is seen the special productiveness of the idea, beyond, for example, the idea of a series or of any complex combination where no moreness is thought of. For these latter do not have in themselves a tendency to stimulate thought, because they do not give us a consciousness *that there are relations* waiting to be described. In any series, it is true, there *are* such relations, but nothing tends to make us *aware* that there are. But *more*, being a *relative* word, is the same as a statement, "A relation exists here." And being also a word implying a content (a size) it also states, "The relation has a character, for it holds between terms with definite characters." Here is seen the second peculiar quality of the idea; that it is not merely relative, but dual, in that it has a content of its own to which it is relative.

Let it not be said that the whole procession of ideas above described can be fully accounted for on grounds of memory and instinct. It is true that without memory I could not recognize the original in the later elements of the series, and therefore could not get the idea of sameness. It is also true that unless I had been endowed with an instinct of curiosity, I should not desire to conjure up new names for the original more-series. This, however, does not prevent the train of ideas which arise from being a causal train. Memory is but a name for the way in which certain contents appear—*a classifying, not an explaining* name; curiosity, likewise, is but a name for many similar ways of feeling which we experience before the discovery of new materials. It matters not how they arose, nor whether they depend on physiological conditions for their possibility. The essential point of my argument is that when they do come into play in the manner described above they appear as names describing a causal process, comparing the way we feel (in case of curiosity) or the physiological conditions which make its action possible (in case of memory) with the way we often feel at other discoveries, or with the usual physiological conditions of a consciousness of a series.

3. It remains to state the rest of the process, showing thereby

that the original idea *more* leads naturally to ideas of number and quantity.

3d Idea.—*More* has now been defined as the relation between repeated lumps of size (increments of any kind) which are the same and yet different. But sameness + difference is insufficient. Sameness did not describe the relationality of more—it led the mind into the character of each increment—and now difference fails to tell us anything *about* moreness. When we are told that any two things differ, it is much the same as if we had been told that they are related somehow—difference is so very general a term. We at once ask, how do they differ? To describe *more* we need a third idea, telling the difference between the members. This idea is position in the series. Position is here defined as "That in which the repeated increments in a more-series differ." But, again, no new content has appeared, for the definition is a circular one.

4th Idea.—*More* is now described as the same thing over again in different positions along the series. But this is unsatisfactory, because it is not a relation between the sizes, but only between their positions. It does not tell all there is to tell about *more*. *More* appeared to us at first as a relation between sizes, and though we have been trying to tell what this relation is, we have actually been describing only aspects of it. It will, therefore, continue to constrain us. So we ask, what is the relation between the sizes? This gives us a fourth idea, *sameness in size* (corresponding to difference in position). Size is here defined as "That in which the differently-placed increments in the more-series are the same." It is not the same old idea we had at first (in the original definition of *more*), but means now: Size that may be compared with other sizes. This is not yet measurement, of course—which needs the idea of equality as well—but might be called, perhaps, the possibility of measurement.

5th Idea.—Still the relation *more* is not described enough. Sameness between sizes leaves out of account the relation between the later members of the series, *as later*, and the earlier, and thus tends to make us lose sight of the relation of increase. So that relation which keeps urging us to describe it will drive

us further to characterize the relational element, *i. e.*, to describe difference in position. What is the relation between positions? The answer is, later and earlier as corresponding to more and less. So we get as a fifth description: A series of same sizes which are *more* as they come later in the series. Here we have brought in the notions of *later* and *earlier* as describing *more* and as comparing it with its own part, that which is *less*.

6th Idea.—Since, again, later and earlier describe only positions, not sizes, we must ask for the relation the *sizes* bear to one another, *as sizes*—not as in this or that position. And here the answer is, sameness that is not moreness nor lessness; or, better, the equivalent word, *equality*. This has gotten before our minds the idea of a finite number-system. We have a series of equal sizes, which series increases as new members are added.

7th Idea.—But have we even yet described the relation which connects the more with the less—or one increment with the next? We want to find the bond of connection between them, that is, *more* says once again to us, "There is more about me than you have seen." So now we will describe this new part, connecting any two increments, in our usual circular way, as 'that about the relation *more* which is *more* than we have seen.' In so doing we view the new part as a *more*, and it is related to the already present contents as more; therefore it suggests, just as the other one did, another element in between which again shall be more. Thus the relativity of *more* necessitates an explicit process of self-repetition. And as each one of these new "*mores*" has the same nature with the first, it follows that each one involves a 'same size repeated,' that is, the finite number series we had becomes an infinite number series.[1]

We have thus on our hands an infinite series of *mores*, which are all *more* than those preceding, and thus suggest a definition for each number as related to those preceding it and so on indefinitely. This investigation of the possible relations between numbers, however, gives rise to arithmetic, algebra, etc. The

[1] This infinite series is here seen to be an attempt to describe the infinitely numerous parts existing within the originally perceived and finite *more*. This explains why a quantity is a completed infinite.

necessity in these sciences will be due to the fact that number-relations are descriptions of an original material (something that increases) which bring in no new content, yet are the product of a natural movement found in the idea of more—a movement of infinitely complex self-repetition. And this movement is a psychical causation.

STUDY OF EARLY MEMORIES.

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A study of the earliest memories of childhood cannot be otherwise than helpful in giving us fuller knowledge of the child-mind; and hence it has a direct interest for the student of pedagogy. Studies of earliest memories have been made by Victor and Catherine Henri, and the results of their investigations are published in the *Popular Science Monthly* (Vol. LIII.).

Of 123 persons, principally French and Russians, they found that 120 had memories reaching to years of infancy; 20 recalled several events of the same period of time. The ages varied between wide limits, the average being from 2–4 years. Most of the events remembered were out of the ordinary course of affairs and had affected the child strongly, though a few had recollections of common events recalled with striking detail.

Colgrove, in his book upon 'Memory,' devotes a chapter to 'Individual Memories,' studied with respect to the periods of life to which they belong. He finds that for the period from 1–5 years, males have the best memory for repeated occurrences, people and clothing; females, for novel experiences and single impressions. From his whole study he concludes that the facts remembered are those in harmony with the general psychical life of their period.

The present study is based upon the earliest memories of seventy-five of the students of Mount Holyoke College, and of twenty-five students at Yale. Each one was asked his or her earliest memory, and the age at which the event took place. There are some difficulties in making a study of this kind, which should be mentioned here, since the manner in which they are met makes a difference in the final results.

1. Is the memory given truly a memory, or has the person

relating it heard the event described so often as to come to believe he remembers it? In nearly every instance the person was asked if he had heard people talk about the occurrence—and if the answer was in the affirmative, note was made to that effect. Many whose remembrances were of events heard about later could describe details. For instance, one remembers that when she was one year and ten months old her father and mother took her on a journey with them. She cried on the cars and her nurse tried to quiet her. Of this she had been told, but she remembers in addition that the nurse had a blackbird perched on the brim of her hat, which frightened her whenever the nurse shook her head. Such a memory was not thrown out in the average of final results, while those with less circumstantial detail were. Many memories were of events which from their insignificance were not open to a doubt as to their reality. Such a one is: "I remember one evening in early spring standing by the table and having a piece of frosted cake. The doors were open and it was warm and pleasant."

2. Fixing the age at which the event took place. In many cases this can be done only approximately, and there is a large chance for error here. Some can be definitely fixed, as for instance the birth of a sister. Where the age given is very young, the memory was not counted in the final estimate unless definitely determined. This was done in all cases. The earliest age given was a little under one year and the memory was of the person's mother taking a little cloak from a drawer and putting it upon the child. Upon inquiry being made it was found that this cloak was worn to the wedding of the child's aunt, and thus the date of event was fixed.

3. Many people give a first memory and then think of another which may be earlier, or of several about the same time. In this case the one most distinctly remembered was chosen.

Besides the liabilities to error arising from these sources, it must be kept in mind that the questioner cannot always get at the salient points of the memory; or the circumstances under which the event occurred; indeed the subject himself often cannot give the latter; that the number of memories studied

is small, and that in the comparison between the earliest memories of men and women the number of men studied is considerably less than of women—in short, that the study could not be made under ideal conditions. However, the results are, I believe, approximately correct.

The results of the study were tabulated under seventeen heads. The memories were classified; first, with regard to the frequency of occurrence of the remembered event; second, the nature of the event, whether of itself likely to make a strong impression upon the child; third, the remembrance of very minor details; fourth, sense types; fifth, the relation of self to the event. Several other kinds of memories occurred so frequently that they were classified also.

Memory Type.	Total No. Instances.	No. Women.	No. Men.	% Women.	% Men.
Single occurrences	62	55	7	73.3	28
Repeated occurrences	23	17	6	22.6	24
Repeated occurrences but first instance remembered	12	1	11	1.3	44
Extraordinary	32	24	8	32	32
Minor details	55	52	3	68	12
Visual	21	19	2	25	8
Auditory	1	0	1	0	4
Gustatory	5	5	0	6	0
Self principally involved	75	55	20	73.3	80
Self not principally involved	11	9	2	12	8
Pain	9	7	2	9.3	8
Persons	4	3	1	4	4
School	7	1	6	1.3	24
Dress	12	7	5	9.3	20
Birth of brother or sister	4	4	0	5.3	0
Learning to walk	5	4	1	5.3	4
Learning alphabet	2	0	2	0	9

Examples of memories classified as single occurrences are, 'sitting one afternoon in my little chair and pretending to read,' 'being carried home from church,' 'breaking sister's doll,' 'slapping baby sister'; of repeated occurrences, 'playing with my brother,' 'my pet horse and driving in the country.' 12 per cent. are a class which it was thought best not to group with either of these, since they have the nature of both. Such are, going to school for the first time, and the first kilts or trousers. Of course the first day in school happened but once, but the continued attendance at school may have

served to deepen the impression which otherwise would have faded away. Three memories could not be classified upon this basis, because of insufficient knowledge of the conditions.

Examining the table, we find that 73.3 per cent. of the women and 28 per cent. of the men have memories of the single occurrences. Of the repeated occurrences, 22.6 per cent. of the women and 24 per cent of the men, while 1 per cent. of the women and 44 per cent. of the men have memories of the third class. These results agree with those of Dr. Colgrove mentioned above, that for this period the memory of men is better for repeated impressions, and of women for single impressions.

It was found that 68 per cent. of the women remembered very minor details and events of seemingly no importance, while but 12 per cent. of the men had memories of this character. For example, one gives for her earliest memory walking to church with her father, and taking hold of his hand to cross the street. One remembers when three years old trying to wipe dishes for her grandmother. When told that she was not doing it right, she turned and said, "Some day I'll be grandmother and you little girl. Then we'll see!" She remembers just where she stood and how everything in the kitchen looked. Another has just a glimpse of a sunny autumn day at her grandmother's. She remembers being under an apple tree beneath which were many mellow apples, and can recall the fragrance of the air and her feeling of happiness. Although she has not been there since she can describe the place perfectly. The little incident of getting into a carriage is the only memory one gives of a visit made when two and one-half years old. One, who remembers learning to walk, says that she wore a blue wrapper and came through a door. An interesting fact in connection with one memory is that the walk home after the first day in school and the red ribbon upon the broad brimmed hat worn then is distinctly remembered, while the school itself and the other events of that important day are wholly forgotten.

Often figures in curtains, or carpets, and the design upon a cup are described. One man gives as his earliest memory his

first kilts with gilt buttons. We cannot know, of course, how important the most trifling details and occurrences may seem to the child and so cannot estimate rightly.

Taine says: "The primitive impression was accompanied by an extraordinary degree of attention, either because it was horrible or delightful or entirely novel and surprising and out of relation with the habitual current of life," and the observations of Henri bear out this theory; but with the data from which this study is made this is not found true. Only 32 per cent. of the whole were memories of extraordinary events. Many events may have been of that nature to the child which do not appear so now, but an action such as carrying a puppy in a fold of the apron around the corner of the house could hardly be classified as such without knowing more of the circumstances, and the majority of these memories are of as trivial a nature.

Other examples of this kind are, 'lying in bed and seeing my little brother in mother's lap,' 'sweeping the front porch with a little broom,' 'sitting at mother's feet and seeing her do patchwork,' 'the door of a house at which I visited—a dog lay before it,' 'a green lounge in my grandmother's room,' 'seeing mother make pies,' 'peeping through a chink in the door at my brother,' and there are many others, some of which have been given above.

Among extraordinary events are grouped, 'birth of sister,' 'grandmother's funeral,' 'falling off a ladder,' 'having picture taken,' 'going on the cars,' 'being frightened by my great-aunt's pointing a knitting needle at me.' Such memories as these answer Taine's description of being out of the habitual current of life.

As to the sense type to which these memories belong, 21 per cent. were classified as visual. The impressions enter in nearly all cases partly through the sense of sight, but in this group were placed only those in which the *seeing* is made the principal thing. Such were, 'a bed of tulips in the square,' 'the green grass and trees seen when riding,' 'a glass mug on the window-sill when I had the measles.'

Auditory memories are rare, only one being given, the hear-

ing of the story of David and Goliath. This seems strange when we consider how many stories and lullabies children hear. Five mention things to eat, such as candy, cake, 'the griddle cakes with brown sugar at grandma's.' These were classified as gustatory, though that term here does not mean that the taste is actually recalled in the memory.

Motor impressions were not separately classified, but it is to be noticed that nearly all of the memories are of actions, and of events in which the child itself is principally involved. Only 11 were found in which the self was not intimately interested, 12 per cent. of the women and 8 per cent. of the men.

Nine remember feeling pain of sickness or accident. One remembers the smart resulting from plunging the hand into a cup of hot tea. She was one and one-half years old at the time, and has described the details accurately to her mother who did not believe she could remember it. None described feeling unusual pleasure, but most of the memories are of pleasant events.

Persons outside of the immediate family are seldom remembered, and these only as taking part in some action. Two examples out of the four are noteworthy. One remembers the face of a gentleman who stooped down to tell her, as she sat upon the floor, that she had brown eyes like some one he knew. The second had in her infancy been a favorite with a gentleman who went away from town when she was two years old. Several years later she saw him at a party and recognized him, although she did not know his name nor had she seen his picture.

Six men and one woman remember the first day at school. The small percentage of women giving this as their earliest impression may be due to the fact that the average age of the first memory for men is 4.4 years, while for women it is 3.01 years, which is younger than the school age. Five remember learning to walk; two, learning the alphabet; 9.3 per cent. of the women and 20 per cent. of the men remember articles of clothing. It is curious that only one from the seventy-five women gives a doll as her first memory.

DISCUSSION AND REPORTS.

THE RELATION OF EMOTION TO MATHEMATICAL BELIEF.

It has become quite the tendency to make a distinction between standing and current opinions and beliefs (Le Bon, 'Psychology of the Crowd'; Professor George Vincent, in University of Chicago lectures, 'Public Opinion'). The standing beliefs are those that endure unchanged during many generations and the current opinions are those that change with the days.

In connection with these distinctions it is suggested that the emotional concomitants of each of these are different, that while the emotional tinge of the current opinion is slight that of the standing belief is of the most intense nature. While this appears at first true, due reflection discovers that there is a strong exception. This exception is in the nature of mathematical beliefs, many of which have certainly persisted among civilized people a score of centuries. The common faith in the unchanging quality of numbers and the fundamental maxims of geometry are cases in point. It might be said that these opinions only apparently have no emotional coloring; that if you could convince anyone that two and two are not four the person would receive a terrible shock. This is perhaps largely true. But this only shifts the point of emphasis of the problem. The question is not, now, why do mathematical beliefs lack all emotional accompaniments, but, why are mathematical truths so far removed from the range of emotional shock? In the long run the two questions demand essentially the same consideration.

It is to be expected that an explanation of the origin and growth of the science of mathematics would cast some light on the relation of the two factors in question. If it is believed that the mathematical powers of the mind are a growth, then it is necessary to hold that mathematical beliefs are matters of social development as are the other beliefs. If, with Wallace, one should believe that the mathematical power of mind is a new creation, then he is free to hold that mathematical beliefs are not of social origin. The latter position is defective. Anthropologists have abundantly proved that mathematics,

especially geometry, grew up out of human experience, practical avocations. Axiomatic truths were first discovered and proved by practical methods. In its origin, at least, the science or the art of mathematics was a social affair. It is most important here, however, to see that the higher mathematics do not depend on social considerations. As Schubert has indicated (*The Monist*, Vol. IV., p. 294), mathematics as a science is entirely self-sufficient. Other sciences, even the most quantitative and exact, depend on sense-observation. Mathematics is independent of the direction civilization takes. It is deductive in method, as Des Cartes so clearly discerned.

I believe that the perception of the nature of mathematics, the independence of the science of the facts of other sciences, promises to furnish a clue to our query as to why the truths of mathematics are so little subject to emotional valuations. In order to develop this clearly it will be necessary to consider the relation of beliefs in general to the emotions.

When we attempt to classify beliefs we discover we have invaded the domains in which the master minds of modern times have contested with each other in their effort to establish a legitimate and accurate hierarchy of the sciences. For, the development of beliefs in the unfolding of society has given rise to the differentiation of the fund of knowledge into its separate and distinct spheres. Science is merely the authentication and defining of beliefs that have gone before and lighted the way. It would be of little importance to our consideration now whether or not Comte's classification of the sciences according to the principle of generality could be substantiated, or if his hierarchy expresses the historical order of those sciences. This paper is rather concerned with discovering the order which the great beliefs of society assume in accordance with another and different criterion. The problem here is to determine the gradation of beliefs when examined with reference to the amount of intensity of their concomitant emotions.

Of the possible schemes for the classification of opinions that of De Greef (Introduction à la Sociologie, Vol. I., p. 214) recommends itself to us most highly. De Greef erects seven categories of beliefs in the order of their depth of intensity. The classification is also intended to correspond in a degree with the order of their social genesis. His order, grading upward from the more to the less intense, is as follows: economic, genetic, esthetic, systematized beliefs (such as religious, scientific, etc.), moral, judicial and political. It is intended to include in each only the great fundamental opinions.

Some such order might be accepted, with modifications, by which to mark the grades of opinions as measured by emotional expression or reaction. Let emotional reaction under attack be the criterion, and in general the strongest reaction would denote the opinion about facts which stand closest to life processes or are so considered, those that seem most essential to the support of the grade of life which entertains the beliefs. Then of the general standing opinions, the deepest and most persistent would be precisely those that have lived longest in the race and are the most strongly imbedded.

It may seem indeed that from the simple fact that the beliefs are so old we have an explanation for the intensity of their emotional concomitants. But this would be a mistake. Upon the general basis of Baldwin's showing, it is clear that age has to do with it only in getting the ideas imbedded in the creeds, institutions, ceremonials, etc., so that it is certain to be impressed on the new individual of successive generations. The fact is, the old ideas have the advantage over new ideas only in having the social institutions and instruments of propagation behind them. They go on living in society by reason of the persistency with which they are dinned into the ears of children in the home, the school, the church. Then when you ask why they become established in the first place, I consider I have given the correct reply above, namely, because they stood close to the interests of life in some way.

This position is quite different from the one that makes the emotion in connection with such ideas the outgrowth of time. That view holds that simply from having believed the same thing for generations that thing becomes instinctive to the individual born into the race, as it were, and the emotion is of the instinctive order. I cannot believe that this is a true statement of the case. This would be only the reinstatement of the innate idea theory. We know of no hereditary ideas. Transport an infant of the most monotheistic and monogamic people on earth among polytheistic and polygamous races to be reared, and the parental ideas would not appear but the child will grow into the ideas of the strange people. In deaf and dumb children no inherited ideas have been discovered. In the changes of fashions which occur frequently there may be quite as great an emotional shock over the discovery by a society dame that her gown is out of style or order, perhaps a much greater one, than over the thought that her morals are askew. Ideas of fashion cannot be hereditary. Emotional discharges there follow common channels for a variety of stimuli, as in other cases.

If, then, it is true that ideas are handed down only by social process but not by heredity, it would at once appear that the emotion and idea cannot be connected in hereditary transmission. That is to say, the emotion has no special label before the fact in connection with it. The emotion is free to connect itself with any appropriate idea or situation which is presented. Theological beliefs, for instance, will awake strong feeling according as they arouse a situation of danger to the individual or as they are made to his mind to seem to be essential to his existence. Why should the average Christian feel shocked to hear the doctrine of the godship of Jesus attacked? Because he has been taught to think that Jesus as God stands between him and an awful torment hereafter. The whole position occurs as a terrible image upon entertaining a bare suggestion to the contrary. No one wants to lose his life. So with the other beliefs in order and proportion according as they have injurious situations connected with them to be rung up at order.

Instead of grading the great beliefs by rank according to age alone, therefore, I should classify them according to their ability to prove their clearness to men by their emotional coloring. The fundamental one is the belief in the right to live. The next in order is that of the right to means of subsistence. The third class is the genetic beliefs. Then come the religious, ethical, governmental, legal and scientific. This scheme might claim justification also on historical grounds. That is to say, the order has been that historically construed in a broad way. In some particular generation or locality or individual development, however, some other order may be the one.

From all that has gone before it may not seem strained to suggest that we now have a foundation for constructing an answer to our initial query, namely, why mathematical ideas, beliefs, are so remote from any liability to emotional coloring. When treating of mathematics I indicated the deductive nature of the science and how independent are the higher mathematics of ordinary affairs and of other sciences. Only the beginnings of the science were facts of sense-perception from which to make induction. Now this attitude of remoteness of mathematical truths, this aloofness from the vital affairs of society, precludes almost wholly the possibility of their bringing into mind the image, the situation of danger or otherwise. Let the truths be doubted or attacked ever so fiercely and there is no picture of a fiery furnace, or starvation, or a disrupted home or starving children arising from it. These truths rather stand outside the range of the fiery and burning interests of men. They seem to be mere instruments to use in connection with affairs,

like guns, or carriages, but there is nothing about them to fear or to laugh at, or languish over. As beliefs they never appear to be my personal beliefs.

This aloofness, this exclusiveness from the tug-of-war of heated, intense, vital matters explains, in my estimation, the lack of emotional accompaniment of mathematical truths. So, in the graded series of beliefs mentioned above, the mathematical beliefs should be placed after and above all the other beliefs when measured by emotional accompaniments. Thus they stand more distant from our warm personal interests, at least to the masses of the people, than do scientific tenets.

J. M. GILLETTE.

REDREAMING DREAMS.

Several years ago I had a peculiar experience in dreams, in fact so peculiar that I made a note of it at the time.

More recently I had another, a similar experience, and I am now prompted to embody the whole in a short paper. I am led to do this partly because I see nothing like it reported in the literature within my reach, but chiefly on account of a concluding remark by Maudsley on 'Sleep and Dreaming' in his 'Pathology of the Mind'; he says, page 49: "Dreams have been a neglected study; nevertheless it is a study which is full of promise of abundant fruit when it shall be earnestly undertaken in a painstaking and methodical way by well-trained and competent observers. To physicians of all men it is likely that they will prove full of instruction."

Early in December, 1894, I became severely afflicted with an inflammation of the upper air passages, including pharyngitis and tonsillitis. There was considerable fever, my temperature rising to 103 degrees, a rapid pulse and loss of appetite. In the evening I became very restless and slept but little at night, being awakened at short intervals by, or with, disagreeable dreams. I occupied a warm and well-ventilated room at the time. The medical treatment during the day, as well as the day following, consisted in taking, with some food, ten to fifteen grains of salol every four to six hours, with enough codeine to allay the cough. I may add that I am six feet tall, strong and healthy, a little over thirty years old, and have at different times taken salol in similar doses but at longer intervals and always without experiencing any disagreeable effects. Moreover, I constantly dream while asleep, but a dream is seldom so vivid that I can recall it distinctly the next morning.

On the next, or second, day the local and constitutional symptoms were more marked, but in the evening I felt easier and lay comfortably dozing on the bed until 10 o'clock when I undressed and retired. Now my queer experience began: I had a short, vivid and coherent dream in which I was carried back to the days when I was an interne in a city hospital. I dreamed that the patients were not properly cared for in going to and coming from the general dining-room, there were too many decrepits who should have taken their meals in the wards, or even in bed, and who had great difficulty in following the procession up and down the stair-way. The scene and the characters stood out distinctly and vividly. The dream was very disagreeable; it excited and worried me, although it had no basis in facts, and besides, the feeding arrangements were matters with which I had nothing to do. I remained awake for some time thinking it over and the falsity of dreams in general, and then fell asleep again, but only to be awakened in a short time, disturbed and excited by the *same dream*. There was no variation in the scene nor in the faces. I was more excited and annoyed than before and tossed about until finally I was once more lost in sleep. In a few minutes I was again awakened by, or with, the same annoying dream. To make the story short: *I dreamed that same dream at least twenty times in succession*, and all in the course of about two hours and a half. It remains to say that every time I awoke the throat had to be cleared of the accumulated abnormal secretions and the gargle used. The secretions were certainly sufficient cause to account for my frequent awakenings independent of the dreams.

Shortly after midnight I got up to take a fifteen-grain dose of salol, with codeine. I was all out of humor and wondered if this sort of thing was going to continue all night—and a night never seemed longer. I remained awake, tossing about, for half an hour perhaps, and then became unconscious in sleep. In a short time, a few minutes apparently, I awakened: I had had a *new dream*. The scene was now shifted to the hospital where I was at the time (Longcliff) and to a ward where the feeble and incapable are cared for. The subject was similar to that of the first dream, or rather, set of dreams: I saw that the patients were not properly fed and nursed. I was greatly distressed and excited, yet on thinking over the matter I saw how utterly false the dream was. Now, strange to say, I redreamed this also; I awoke at least twelve or fifteen times and was always annoyed by the same dream with only slight variations. Occasionally there was some incongruity in the scene, but the few charac-

ters always appeared distinctly. At 4 o'clock in the morning I took another ten grains of salol.

Although feeling very ill at the time, I was nevertheless much impressed by the peculiarity of this kind of dreaming and frequently consulted my watch to note the hours of the night, always wishing that daylight would come and put a stop to it. That the matter was not simply one grand dream, or illusion, if you wish to call it so, was evidenced by the fact of the gargling solution in the bottle being nearly consumed during the night. I must have gargled at least twenty times during the night judging by the amount of solution used. There still remained over two hours before dawn and I began to reason that if I must redream I might as well have a pleasant dream. I had frequently heard it said that a person could to some extent influence the character of his dreams by directing the thoughts to some subject previous to falling asleep, and I tried therefore to keep my thoughts on pleasant things, and finally dropped asleep. To be brief: *I had a third set of dreams.* This one was a pleasant one, or set, and I awoke six or eight times from it. After 7 o'clock I had a deep and refreshing, apparently dreamless, sleep lasting two hours. On getting up in the morning I was able to clearly recall the first and second sets of dreams, the sleepless intervals and the taking of the remedies, but the subject or nature of the third set had vanished; all that I could recall was that the dream was a pleasant one and did not worry me, and that there was some incoherency in the scene and characters. My temperature at this time had fallen to 100 degrees and I felt comparatively well, although rather weak. The following night was uneventful; the disease-symptoms had practically disappeared.

The above account is essentially as written at the time. I will now give my second experience, which occurred two years later, or, to be exact, on the early morning of February 3, 1896.

For about ten days previous to this date we had an epidemic of influenza; many of the patients and employees were coughing and sneezing and complaining in general. I too became affected and after trying various remedies without any decided good results, finally, on the evening of the 2d, took twenty grains of salol and one-fourth grain of codeine. I slept fairly well during the fore part of the night, but the nares being stopped up I was compelled to breathe through the mouth. Soon after midnight I had a very vivid dream. The subject was a puzzle, or game: A board measuring about 9 by 14 inches had two large holes in it lengthwise; above these was a row of about ten

or twelve small holes for pegs, arranged in the form of a crescent. In the lower left-hand corner were two additional peg holes. This part of my dream was very distinct. The modus operandi, as I remember it, was like this: A sentence, a quotation for instance, was to be thought of and the chief words, or the subject and predicate, were to be written on a slip of paper and placed back of the large openings. A peg to indicate the affirmative or negative of the sentence is inserted in one of the other lower corner holes, and a number of pegs equalling the remaining words placed in the crescentic row. The board thus prepared—fitted out with the two chief words, an affirmative or negative peg, and a peg for each other word—is held up to full view for a second or two, the company or guessers grasp as much as possible and then begin to construct the sentence. The board is then passed to the person giving the first correct answer.

The subject was a fascinating one and made a deep impression on me. A few days before this I had read of a man who 'got up' puzzles, or games, and who often received several hundred dollars for a new one. In my feverish condition visions of hundred dollar bills floated through my brain, and this, perhaps, had something to do with the redreaming. I dreamed it over and over and each time it had a charm. The next morning, however, it did not look so promising; the idea, though, was new. Soon the thought of having taken salol came to mind and I recalled my experience of two years before, as related above, and in the light of it I wrote out the present account.

I have briefly related the bare facts and shall not attempt to draw any conclusions. It seems to me, however, that the large and frequent doses of salol, over two drams in the first case, may have stood in a causative relation to the dreams, that is, to the redreaming.

Ordinarily we seldom give a second thought to a dream—"It is only a dream" we are apt to say. In the present case it is not the dreams themselves that are peculiar; the peculiarity consists in the *redreaming*.

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PSYCHOLOGICAL LITERATURE.

La suggestibilité. Par ALFRED BINET. (Bibliothèque de pédagogie et de psychologie, III.) Paris, Schleicher Frères. 1900. Crown 8vo. Pp. 394.

The aim of this book is to present the facts of normal suggestibility without reference to hypnotism or pathological conditions. The author has set himself these two problems: Is it possible to determine the individual suggestibility without having recourse to hypnotism? And, what are the relative merits of the tests proposed? The first eighty pages are devoted to an historical review of the subject, and the remainder of the book consists of original reports upon researches made personally by the author upon school children in Paris.

The author distinguishes the following classes of suggestion: (1) Obedience to the moral influence of another. This is the technical sense of the term. (2) The tendency to imitate. This may combine with the first but may also appear without it. (3) The influence of a preconceived idea which checks the critical sense. (4) Expectant attention, or unconscious errors of an ill-regulated imagination. (5) Subconscious phenomena produced during a state of distraction, or following an event which has created a division of consciousness, as in the case of automatic actions. He rejects the current loose uses of the term, such as to denote "the idea passing into action." In the narrow sense suggestion is a moral pressure which tends to produce an automatic state of consciousness. It may be effected through fear, love, charm, intimidation, respect, admiration, etc. This automatic state is analogous to the state of *rapport* in hypnosis.

Experiments are reported upon the following topics: the directing idea; moral influence (action); the question; imitation; and, subconcious movements. An individual study was made of each child by both objective and subjective methods. The children varied in age from seven to fourteen and the experiments were made in the office of the principal of the school with apparently no more disturbance than an examination in arithmetic or geography would have created. The report is dramatic and carries the reader through a lifelike experience face to face with the child. The tests may be mentioned *seriatim*.

The Directing Idea.—Here an attempt is made to eliminate the moral, personal influence and to develop autosuggestions. The tests are planned to throw light upon the trustworthiness of observation and memory under the influence of prejudice, *parti pris*, tradition, controversy, old fogyism, etc. Ordinarily the course of our daily life is determined by the presence of certain guiding ideas. Here concrete life is reduced to experimental conditions, and the origin, the course, and the efficiency of one idea is studied. There are several tests in the series. First, the child was required to mark off, on cross section paper, the length of lines presented by the experimenter. The lines were shown in the following order, the length being given in millimeters: 12, 24, 36, 48, 60, 60, 72, 72, 84, 84, 96, and 96. The order of the first five lines invokes the idea that there is a systematic order of increase in the series; after the fifth, the alternate lines are catch lines. The directing idea led first to the assigning of an increment to each of the catch lines, and consequently to a corresponding overestimation of the last lines in the series. On the basis of the records a 'coefficient of suggestibility' is computed for each child. These coefficients vary from 7 to 129.

These results suggested another test, namely, to what extent can the directing idea increase the apparent length of a line for each individual child? The children were asked to mark off the length of lines as before. This time thirty-six lines were shown. The first five were the same as before but the last thirty-one were all of the same length, 60 mm. This is a superior test. The order of the first five lines established the directing idea, and the manner of lengthening the following lines furnishes a measure of the suggestibility under these definite conditions. Eight out of the forty-one pupils continued to increase the length of the lines up to the end. One automaton made the last line 304 mm. long. The force of the directing idea is enormous. In general, there is a pretty close agreement between the individual coefficients in the two tests. By very ingenious questioning each child was led to give his introspective interpretation of the process. The majority were aware that they had made the lines too long, but when asked to mark the supposed errors, they corrected the overestimation only in small part. Many different, true reasons were given for the committing of the error. The introspection throws some light upon the mechanism of the directing idea, and the author is convinced that it is a legitimate and profitable method to employ in the study of children. He also thinks that tests of this kind are very helpful to the child, who thereby learns to form correct habits of observation and

action. This test was repeated upon twelve pupils of a higher grade in the school, and the results show that the older children were less suggestible than the younger.

A third test was made to determine whether the suggestibility found in one sense will be the same for another, *i. e.*, whether suggestibility is a general process of the mind. The test was made on discrimination for weight. The apparatus consisted of fifteen small boxes of uniform size. The first five weighed 20, 40, 60, 80, and 100 grams respectively, and the rest were all the same as the fifth. Three methods were employed in turn. First, the child lifted the boxes in order and stated whether the one lifted was heavier or lighter than the preceding, or equal to it in weight. Second, he lifted as before but verified for each step by repeated lifting of the two boxes compared. Third, the experimenter stated that the first box weighed 15 grams and the pupil then estimated the relative weight of the other fourteen, in grams. All three methods bring out a pronounced suggestibility. A few pupils are refractory; as many accept the suggestion and become complete automata; and the rest are distributed in various degrees between the two extremes. There is an approximate agreement between the individual coefficient of suggestibility for weight and that for the length of lines. This means that suggestibility in one sense experience implies suggestibility through the other senses as well. There are, however, differences in these records which indicate that an individual may be more suggestible through one sense than through an other. There are, therefore, types of suggestibility, just as there are distinct types of mental imagery. While we are furnished evidences of the mechanical action of the mind, the same phenomena reflect the almost infinite complexity and variability of the nexus which constitutes each child's characteristic reaction to the environment.

A very pretty illustration of law in illusion occurs in this test. The figures show that the sixth box is judged lighter than the foregoing more frequently than a similar judgment is given for any of the other boxes. Chance, one may think. No, this fact could have been foretold, because according to the law of negative illusion the greatest disappointment of expectation would occur at this point. Of course it must be taken into account that we have here a conflict between the negative and the positive suggestion.

The tests were repeated upon older pupils and it appeared, as in the case of the linear illusion, that the older children were less suggestible. The author remarks that suggestibility varies more truly with intelligence than with age. The reviewer has on hand data to demon-

strate that this variation with age and intelligence cannot be stated as a general law. There are certain classes of suggestion which diminish in force with increasing intelligence, as in the cases before us, but there are others which do not diminish, and there is a third class of suggestions which gain force by virtue of the intelligence. There is a grain of truth in Berillon's paradoxical thesis, that suggestibility is synonymous with educability.

Professor Binet employed the introspective method to excellent advantage in repeating this test upon his two daughters, twelve and thirteen years of age respectively. He is never content with mere figures. Throughout the work his aim is less the measurement of the extent of suggestibility than the more important study of its nature.

Moral Influence.—In the foregoing experiments the personal element was eliminated as far as possible; here it is the object of study. What this series of tests lacks in exactness and positive control, as compared with the foregoing, is here made up in lifelikeness and practical applicability. How do we influence each other with our words, our inflections, our gestures, our logic, our prestige, our authority? Two lines of experiment are pursued: The first is a test of docility as shown in the accepting or rejecting of an affirmation; the second is an experimental analysis of the personal influence through questions.

The affirmations are of two kinds, contradictory and directing. The former is taken up first. The experimenter asked the child to name each of the spectral colors shown to him. Then a series of nine colors, some pure and some mixed, were shown and as soon as the child named the color the experimenter contradicted him and said, No, it is so and so. The children were more refractory in this test than in any other. A much better test was made on the same point with affirmations about the length of a line. This is superior to the other test because the contradiction is not so offensive and it is more exact. A series of twenty-four lines, varying in length from 12 mm. to 104 mm. in an arithmetical ratio, was placed before the child. A standard line was then shown and the pupil selected the one that matched it in the series. Immediately the experimenter interfered with: Are you sure? Is it not this (pointing to the next line above)? Most of the children accepted the suggestion.

In the next series a directing suggestion was given before showing the line. The child was asked to mark off the length of a line 60 mm. long, shown to him. Presenting a second line of the same length, the experimenter said, "Here is a longer line." Then present-

ing a third line, he said, "And here is a shorter line." In this way he alternated the suggestion for a number of equal lines. Most of the children accepted the suggestion. The author does not take adequate account of the negative influence. The suggestion he gave was at once a motive for two opposing illusions. If the bright child actually believed that the third line should appear shorter than the second when seen, this third line ought by virtue of the definite expectancy to appear longer instead of shorter. It is not sufficient to say that those who do not accept the positive suggestion are refractory. The records represent the resultant of opposing influences and may be predominatingly positive or negative. When the two forces neutralize each other we have true perception, as far as these elements are concerned.

The Question.—How can a judge influence the witness by his questions? He may allow the witness to exercise complete spontaneity; he may force questions without biasing; he may make leading suggestions to the witness by the questions; and finally he may force extreme suggestions by questions. Here is a vast field for investigation. The author reduced these cases to experimental conditions for children and tried to work out the mechanism of the personal influence of the question upon a child.

A sou, a button, a stamp, a label, a photograph, and a small picture of a crowd were pasted upon a large card. The first experiment indicates the second attitude of the judge, as stated; it is the case of forced memory. The experimenter showed the object card twelve seconds and then, as the child reported what he had seen, he asked forty-one questions about actual details in the objects. The results show that a full and concise report may be made, and yet be false, *e. g.*, when a child sketches the cancelling letters R I S on a stamp that has not been cancelled at all. The great lesson from this excellent test on observation and memory is that definiteness and completeness of detail are not incompatible with falseness. Reliability is not correlated with ability to recall. There is a characteristic specialization and concreteness in the errors that is traceable to the questions. The errors are of two types: namely, logical errors, *e. g.*, having stated that the button is sewed on (it is pasted on), the logical error of assigning a color to the thread follows; and, errors of invention, *e. g.*, when some fancied object is described instead of the real. The pupils who were most suggestible in the former tests made the greatest errors.

The first case of the judge was imitated by repeating the same test

upon other children without asking any questions. The children simply wrote the answers as fully as they could. Their reports were less complete and numerous errors were made. The author concludes that if you desire faithful testimony from a child you must not ask him any questions, nor allow him to make oral report, but require him to write spontaneously what he knows. This is especially true about children because they are less capable than adults to distinguish between fact and fiction. But how many adults are not grown up children in this respect!

Following this, three tests were made by the use of written questionnaires. The same objects were shown as before. A number of children were divided into three groups and each group received a different set of questions. The first was intended to force memory, *e. g.*, "How is the button fastened?" The second gave a moderate suggestion about each object, *e. g.*, "Is the button fastened with a thread?" The third gave a strong suggestion, *e. g.*, "What is the color of the thread which passes through the holes of the button and fixes it to the card?" Thus, the last three cases of the judge are produced. Each set of questions brought out characteristic results. We are presented with a classification of these little witnesses with reference to the truthfulness of their testimony. The majority accepted the suggestions and wrote as if the memory images had been true and spontaneous. This feeling of spontaneity is an exceedingly important factor which was well brought out in the introspective reports. The tests were repeated upon some normal-school students. These adults made the same kind of errors as the children had committed, but not so gross. These students were undoubtedly more reliable observers than men ordinarily depended upon in courts.

Imitation.—The same object card was used as before. Children were called into the principal's office three at a time. One in each group was appointed president-secretary, whose duty it was to read the questions and write all the replies. A definite list of questions about the objects was furnished, and as soon as a question was read the three competed to see which one could answer first. The order in which the three answers came was recorded. Naturally there appeared a leader in each group, and the test consisted in determining to what extent the other two followed this leader. The results are astonishing and lend reality to some of the strange accounts of mob action. This introduces the psychology of contagion. The group is more suggestible than the individual. Each group, though formed by chance, has a group character or type. The imitation is very pro-

nounced. But the suggestion is more powerful than the tendency to imitate; many of those who succumbed to suggestion were not imitators of their comrades. On the other hand, a few imitate in resisting the suggestion. The children were regrouped and a similar test was made, with the result that each individual adapted himself to the new group and the laws determined in the other test were corroborated.

Subconscious Movements.—An 'autoscope,' to use Professor B. F. Barrett's term, was made by mounting a hammer so that it would swing up and down gently, supported by a spring. The child held the mallet head in the hand behind a screen and was required to look at a metronome and count the beats. The experimenter 'primed' the movement by causing the hammer to swing in time with the metronome, about ten times. The test consisted in determining to what extent the child would continue the movement automatically after having been thus started. A classification of the children in regard to the definiteness and duration of these automatic movements is given. A second test was made in which a pencil was used in place of the hammer. The experimenter communicated simple writing movements and these were often continued automatically. The children were generally conscious of the movement. These tests demonstrate the possibility of making quick and significant tests on automatism under normal conditions. They also show that automatism of action is not synonymous with automatism of judgment.

This bare description of the tests conveys no adequate idea of the author's ingenuity, erudition, tact and fairness in the manipulation of an experiment and the interpretation of its results. He is a worthy leader in this field of research. He has proved more than he set out to prove. Not only has he demonstrated a relation between normal suggestion and hypnosis, and between normal automatism and the spiritualistic phenomena of automatism by ready methods, but he has contributed much to our knowledge of the child mind and the theory of applied psychology. Instead of injuring the children he claims to have benefited them by explaining the errors to which they were subject and leading them to more accurate habits.

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Psychologie des Willens, zur Grundlegung der Ethik. Von HERMANN SCHWARTZ, Privatdozent an der Universität Halle. Leipzig, Wilhelm Engelmann. 1900.

The work before us is an interesting and in many ways original contribution to the theories of value which have grown up out of psy-

chological studies of recent years. Though in method somewhat akin to the Austrian school, it opposes to the empiricism and relativism of this school a doctrine of absolute norms to which the author himself gives the name of 'Voluntaristische Apriorismus.' By this title he would distinguish it on the one hand from the rationalism of Kant and the intuitionists, and, on the other, from the naturalistic and heteronomous theories of the origin and development of values which characterize the empirical point of view.

The phenomenology of values, as disclosed by the psychological analysis of Schwartz, may be summed up thus: Values fall into three distinct and ultimate classes.

(*a*) Values of organic condition (Zustand), mere feelings and, as such, objects of mere desire; (*b*) Values of the person, such as admirable qualities of the person or character, beauty, fame, valor, etc. (*c*) Impersonal values, social, artistic, scientific, in short all altruistic values.

These last two classes are distinguished from the first in that they are objects of outgoing will activity in the true sense, not of desire. Within each of these spheres of value, preference is determined merely by judgment of like and dislike. This is a purely analytical preference, determined by the degrees of satisfaction (saturation of the volitional tendencies). But the choice of one group rather than another follows a normative law of synthetical preference. There is in both individual and racial culture a tendency to development of motives from the lower to the higher classes, but while the origin of motives may be explained by principles of natural causality the choice of one class of motives over another is explicable in both individual and historical culture only by a synthetical principle inherent in the will. Moreover, the inherent necessity of reason to explain acts of will by sanctioning grounds found in the sphere of the representations which the volition brings in its train, shows this *a priori* principle of preference. In this connection the instinctive lies of consciousness, flattering by false reasons our higher sense of values, are instructive as showing the existence of the norms of personal and impersonal values. These phenomena of ultimate distinctions in values, of semi-logical development of motives in individual and social culture, of intuitive recognition of grades of value by the sanctioning consciousness, all point to synthetic *a priori* acts of will back of individualistic development.

Side by side with this positive programme, the psychological grounds for which constitute the larger portion of the work, Schwartz

carries on a running fire of criticism against all heteronomous conceptions of will-determination. With special energy he maintains that the doctrines that the will is determined by such principles as the 'Law of Relative Increase of Pleasure' (Ehrenfels), the 'Will to Life or to Power' (Schopenhauer and Nietzsche), or the logical principle of 'Universalization of Motive' (Kant), are dealing with mere abstractions, not real forces of the will. In accord with his conception of the primacy of will as the creator of values, which is the keynote to his position, the ground of will-determination in any real sense must be a 'Gesetzlichkeit der inneren Normen.'

The absolute dualism which his theory discloses between the 'Natur-zwang' of the merely natural laws of desire, as like and dislike, and the 'Norm-zwang' of synthetic preference of the will opens a way for a doctrine of freedom which rests ultimately upon the old Kantian principle of the indifference of norms and of nature to each other.

This is in briefest outline the obvious part of our author's system. Our first astonished question is: Whence these ultimate categories of value, whence our knowledge of this 'Gesetzlichkeit der inneren Normen,' which is active back of the naturalistic changes and development of motives? It would be easy to grow indignant over this bold and unmitigated dualism in method. Having shown such zeal against the dogmatism of Kant's intellectualistic ethics, of the naturalistic Trieblehre of Schopenhauer and Nietzsche, and of the pain-pleasure illusion and prejudice of the empiricists, has he presented us with the last consummate illustration of the assertatory method? We read on and on. Surely there is some logical deduction of these categories yet to come. Kant's ethical *a priori* may be but a pale spectre from the shadows of the pure reason, but at least we know its source. We know how to estimate its credentials. But no—a brilliant *argumentum ad hominem* tells us to go to our own will. Does it not choose personal over feeling value, impersonal over personal? And when, in its weakness, it fails thus to choose, does it not sophisticate itself with those lies of conscience which are the tributes paid by vice to virtue? We find nothing but a reconstruction of that table of values, attractive but powerless, which Martineau develops for us in his Idiopsychological Ethics.

Idiopsychological is Schwartz's position throughout, and by his own statement he has much in common with Martineau. And with such reflections and with this classification many readers will forthwith dismiss the book. Such a procedure would, however, not be wholly

just, for unpromising though the result may be there is yet back of it a psychological point of view which is worth notice for its own sake. In his preface we are assured by the author that an adequate psychology of the will is a prerequisite of a true theory of ethical values—a view which we can scarcely doubt, despite the strong reactionary movement in which we have been caught up. But immediately we are brought face to face with the distressing uncertainty prevalent as to true psychological method. Against the empirical method and its concept of the will as a complex of elements, we have in Schwartz the strongest insistence upon the will as a special function of the soul, with its own quasi-logical functions, independent of ideal content. Moral distinctions are neither of feeling nor of thought, but are idiopsychological and idiogenetic.

"Von dieser Antwort," says Schwartz, "hängt für die sittliche Betrachtung unendlich viel ab." Without doubt. If, as Schwartz's analysis would have us believe, the function of desire is merely that of a preparatory process to furnish the material for the analytical and synthetical functions of the will, "reflexionsartige" (judgmental) processes, then indeed the real grounds of the will are not to be found in the empirical doctrine of motives, in representation and feeling, but in the will itself, and the entire basis for a causal method is gone. Naturally, such being the conception of will, the causal and empirical method of psychology is supplanted by a negative analysis, the purpose of which is to show the independence of will of motivation from without.

We find, therefore, a new doctrine of value and a new conception of volitional sufficiency. The so-called motives of the will, representations and feelings, are not of value because they are objects of desire or will, but *become of value* first by reason of the satisfaction (saturation) which they bring to the impulse of will which brings them into being. Even elemental will impulses—and this is the key to the situation—are conceived of as objectless. But these objectless 'Willensregungen,' by a law of concentration of ideas, bring into being a crowd of representations among which analytical and synthetical preference is exercised, on the basis of the satisfaction (saturation) of the impulse. 'Nicht durch das Gefallen oder den Wunsch selber sind wir uns eines Zieles bewusst, sondern wir werden es durch eine Vorstellung die zu ihm hinzutritt und sich nachträglich bildet.' The values or ends of volition may be interpreted or known in terms of representations, but the values consist not in objects of impulse or will, but in the satisfaction of the will that calls them into being.

This novel conception of satisfaction of the will, through saturation of the will impulse, by the more or less of content, the presence or absence of the same, in so-called analytical preferences, within the same sphere of values, and the reaffirmation of the doctrine of practical evidence in the sphere of synthetical preferences between different classes of values, is one, it is to be feared, with which scientific method can do nothing. From the point of view of science the values of the will can be known only in one of two ways: either by the ideal products of the will reconstructed logically or causally, or by the subjective states which are the functions of will activity in consciousness, these to be reconstructed either in terms of degrees of pleasure and pain or perhaps in semi-logical terms of extension and intension of sentiment—from the reviewer's point of view, the latter.

With the introduction of the conception of a purely practical evidence of the will, abstractly conceived, the distinction of Crusius between real grounds and knowledge grounds of the will is carried to its extremist conclusion. But how are we to know the real grounds of the will if they are not to be found in the phenomenal motives? If we do not reconstruct them out of the phenomenal motives, have we not to choose between a groundless will or an arbitrary metaphysical theory of the will? And if the values of the will are to be reconstructed out of phenomenal motives, how shall we escape the logical or causal methods?

Space will not permit of a detailed criticism of the errors of analysis involved in the statement that volition is not consciousness of ends, the most far-reaching of which is the failure to distinguish between different degrees of consciousness of ends. We must confine our notice to some of the logical consequences of the general position which make it so out of harmony with the prevalent theories of value. Having abandoned the basis of the 'continuity' theories of value which define value as that which is desired, and therefore all modifications of value as development of desire, Schwartz's classes of value lack all genetic, phenomenal relationship to each other.

To make an absolute distinction between Values of Condition, Personal Values, and Impersonal Values, he is compelled to use the conception of feeling, 'Gefühlsdisposition,' in an arbitrarily narrow sense, to confine it to the grossly organic conditions of the organism with the strong consciousness of pleasure and pain. These are passive states, *desired*, while the personal values, such as power, fame, beauty, courage (the ethical sentiments in general), are objects of an active effort, *streben*, the value of which is determined by the act of will itself.

The author here betrays an unusually superficial acquaintance both with these sentiments themselves and with the psychology of the sentiments when he ignores entirely the prevalent and probable explanation of their relative independence of subjective conditions, by processes of abstraction, and by the objectification of the social consciousness. And here it may be remarked that a neglect of the social factor in the creation of values is characteristic of the work.

As a consequence of this absolute dualism, an entire group of sentiments, wonder, reverence, sympathy, which represent a more objective reference of consciousness, and yet do not fall into the personal and impersonal group, are described as neutral, and denied all further participation in the scheme of values, although manifestly occupying a transition stage between the lower feelings and the higher sentiments of personal and impersonal value. These values of consciousness are, once and for all, states of consciousness, and a psychological method which shall explain them must be genetic and empirical. It may not be possible to explain the degrees of value and the development of values by the law of relative increase of pleasure, which Schwartz insists is no 'real force,' nor even perhaps by the quasi-logical conception of greatest possible extent and intension of sentiments (Guyau, Krügrer). Nevertheless, it is no help to our psychology of values to tread the abandoned paths of the old faculty psychology, to assume 'real forces' for every relative distinction of value we may make in the content of consciousness. The Kingdom of Science is not to be taken by storm.

As for the general methodological principles which underlie the entire work, we find them leading to a result which puts the science of ethics in the precarious position of choosing between the alternative of a groundless will, or at least one whose real grounds cannot be known, or of a transcendentalism, whose categories not only bring no deduction with them to certify their truth, but whose limits, even, are not clearly defined. There seems to be in the present state of our thinking two methods of value determination with which every thinker should set himself to rights. Both are monistic—and admit of no discontinuity of value. Genetic naturalism conceives of all higher values as expansions and modifications of the lower values of 'condition' determined by the laws of the social consciousness. The idealistic method conceives even the values of condition as well as personal and over-individual values as capable of systematization under the concept of an ideal self. In his absolute dualism between the lower and the higher values Schwartz has run counter to the dearest hopes of both schools of thought, and his work will probably be judged accordingly.

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Atlas of the Nervous System, including an Epitome of the Anatomy, Pathology and Treatment. CHRISTFRIED JACOB. With a preface by Prof. Dr. AD. V. STRÜMPELL. Authorized translation from the second revised German edition. Edited by EDWARD D. FISHER, M.D. With 112 colored lithographic figures and 139 other illustrations, many of them in colors. Philadelphia and London, W. B. Saunders & Co. 8vo. Pp. xix + 218.

This book is in small octavo form and appears as one of Saunders' Medical Hand Atlases, being the eleventh volume in that series. As will be gathered from the title, its scope is comprehensive and it is planned to give, from the standpoint of anatomy, pathology and treatment, a condensed presentation of the main features of the nervous system. Fully two thirds of the volume are given to the plates and their explanation, while the remaining third is text—which covers 202 pages. The text is divided into six parts, as follows: Part I., Morphology; Part II., Development and Structure; Part III., Anatomy and Physiology; Part IV., General Pathology and Treatment; Part V., Special Pathology and Treatment; Part VI., General Remarks.

The first three parts deal almost entirely with the gross normal appearances of the nervous system. All together, however, they occupy only 75 pages out of the total 202. On the other hand, they are accompanied by fifty-seven out of the eighty-four plates which the volume contains. This analysis serves to show the distribution of emphasis within the book.

We shall here examine only those parts which deal with the normal structure, since it is this side of the book which is nearest to the readers of this REVIEW. Beginning with the illustrations, we find that the plates which go with Part I.—on the morphology—are excellent, representing in the colors of the fresh state different aspects and sectional surfaces of the entire encephalon. For the most part the figures are somewhat less than natural size, but, nevertheless, they are both clear and artistic. On passing to the plates which go with Part II.—on the development and structure—it is first necessary to correct a fault of correlation. Among the plates, 15–26 inclusive are given to Part II., whereas in the text, Plates 15–53 are credited to this part. This contradiction is a matter of slight importance, except for purposes of reference. The following statements apply to Plates 15–26 only: These illustrations, which picture the developing nervous system and some histological preparations from the adult, are rather poor work and the coloring in many cases crude. Moreover, in some instances they are both inaccurate and misleading. Take for example Plate 19, showing

the cerebral cortex. By way of introduction it should be said that this is a new plate and one immeasurably better than that printed in the first German edition of this book. There is, however, no hint in the explanation that this figure is schematic in a high degree, although in it the bodies of the cortical nerve-cells are enlarged very much more than is the thickness of the entire cortex and hence they appear in the figure densely packed together.

If one can compare this figure with actual sections or with the plates in Hammarberg's 'Studien,' the misleading character of it will at once appear. That the human cortex was never intended for representation on an octavo page is evident the moment we attempt to make an accurate picture of it on that scale, for in order to reproduce the real relations of the elements, all save the giant cell bodies must be so reduced in size that their shape is practically lost. Nevertheless, the method of enlarging the cell bodies out of proportion to the enlargement of the thickness of the cortex—as here done—is not the way out of the difficulty, for it obscures a feature of the cortex which is fundamental, and with this picture in mind it would be well-nigh impossible for a student to appreciate, namely, that increasing quantities of white matter between the cell bodies are characteristic both for the higher state of development in the individual and for those species which stand highest in the mammalian series.

Again, in Plate 22 the nucleus of the descending, so-called nasal, root of the fifth cranial nerve is pictured in green—the color used for the *nuclei terminales* of the afferent neurones—although it is correctly described in the text as a motor nucleus, and should therefore be blue. In this connection it may be noted that green and blue are the colors used to distinguish the two sorts of cell-groups shown in this Fig. 1, illustrating the nuclei of the cranial nerves. The colors are poorly chosen, for they are hard to distinguish even in daylight, and under artificial illumination become quite indistinguishable. On this same plate a fiber with a node of Ranvier is shown in such a way —when taken in connection with the explanation—as to leave the impression that these nodes are general characters of the fibers within the central system. This, of course, is not the case.

In Part III., which in the grouping of the plates (27–53) is designated topographic anatomy of the nervous system, we have an important series of illustrations, mainly new, although among them and the four next following plates are some copies of the excellent series of wall charts for which we are indebted to Jacob and Strümpell. Plates 28–30 inclusive, to which attention is particularly called, represent—

somewhat reduced from natural size—a series of sections through the human encephalon, reproducing in color preparations stained by Weigert's hæmatoxylin method. The lithographic work, though somewhat coarse in the printing, is perfectly clear, and the series, which extends from the frontal end of the third to the middle of the fourth ventricle, shows the important structures in this region of the encephalon, a region which it is most difficult for the student to comprehend without aid of this sort.

If one does not have access to Nebelthau's, "Gehirndurchschnitte" or some similar work of the more expensive kind, these plates are certainly worth owning.

In this review the illustrations have been first discussed, since we are dealing with an atlas, in which the text is subordinate to the plates. In the examination of the text, to which we now turn, it will be only proper to keep separate the responsibilities of the author and the translator.

As a rule the translator has produced readable English, but in Parts I. and II. he has often been very careless in his statements of facts and sometimes has missed bodily the meaning of the German terms and constructions. A few selected instances will show what is here meant. For example, in describing the lateral choroid plexuses, toward the bottom of page 9, Part I., the last sentence reads: "Here they (the lateral choroid plexuses) are closely applied to the lateral border of the optic thalamus and passing backward finally enter the descending horn where they become continuous with the pia mater *which enters through the foramen of Monro.*" The italics are our own. The statement is manifestly incorrect. On turning to the German—first edition—it appears that "Unterhornschlitz" is the word which has been rendered *foramen of Monro.*

This sort of thing should not be allowed to pass, for certainly the student of neurology will have trouble enough without mixing mistranslations with his ventricles.

Again, in Part II., middle of p. 25, there is this sentence: "Another portion of the epithelial cells in the neural canal—the neuroblasts—develops into germ cells, which send out first a long, thick process, and later numerous smaller arborizations from which the ganglion cells and nerve fibers are formed."

The erroneous derivation of the germ cells from neuroblasts appears in the German text, but the derivation of ganglion cells and nerve fibers from the cell branches is due to a misreading of the German text, where it is stated that the ganglion cells and nerve fibers are derived from the germ cells—a statement much nearer the truth.

To Part II. belongs the description of Plate 22. We have already said a word concerning some of the figures appearing in this plate; but the description itself calls for remark as it offers several examples of carelessness which should be noted. The following statement is made:

"The motor cranial nerves originate in the blue nuclei; the sensory end in the green nuclei. The cells of the green nuclei form the beginning of the sensory neuron." The trouble is with the last sentence, which at the end should read '*central* sensory neurons,' as it does in the German.

In the part of this same description which relates to Fig. 2 the last portion of the paragraph is particularly choice. It runs as follows: "Each nerve-fiber contains as its principal constituent the axis-cylinder, which is surrounded by a medullary sheath of variable thickness, the white medullary sheath divided into segments by the nodes of Ranvier. The peripheral nerves are provided with an additional layer of extreme tenuity, the sheath of Schwann, *situated within the medullary sheath.*"

The German text merely states that the medullary sheath is divided into segments, meaning the segments formed by the Schmidt-Lautermann clefts and the translator has put in 'by the nodes of Ranvier' on his own responsibility. As the description here applies to the medullated fibers of the central system the general statement is contrary to fact. The next assertion, that the sheath of Schwann—the neurilemma—is situated *within* the medullary sheath, is the outcome of missing entirely the construction of the original which states that the neurilemma is found within the limits of the peripheral nervous system—as contrasted with the central.

Part III., Anatomy and Physiology of the More Important Nervous Pathways, is quite free from this sort of defect which we have just been criticizing; it is a very good summary of the more important pathways in outline and from internal evidence one would say that it was not the work of the same person who had translated Parts I. and II.

If we turn now to the original text of Jakob it appears least satisfactory in those parts which relate to histology, to development and to those portions of the system the comprehension of which depends on a knowledge of the development. The statement concerning the choroid plexus and ventricles are in several cases inaccurate and obscure. For example it is stated on p. 9 of the translation, that the third ventricle has no proper roof—the *velum interpositum* serving that purpose. Oddly enough, on the opposite page (p. 8) there is a cut showing the proper roof of the third ventricle very clearly.

In general, it would be very advantageous if the older nomenclature here used could be replaced by the [BNA] and if the more acceptable form neurone were used in place of neuron. As we are not prepared to discuss the parts relating to pathology and treatment we shall leave that portion of the volume entirely untouched.

If called on for a summary statement concerning the portion of the book we have here been examining, we should say as the curate did of his egg, that parts of it were very good, adding the hope that the text may some day be made as satisfactory as the best of the illustrations.

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The Human Nature Club. An Introduction to the Study of Mental Life. By EDWARD THORNDIKE, Ph.D. Longmans, Green & Co. Pp. 235.

It is a significant fact that a number of the books on psychology which have recently been published in this country have been prepared with a view to attracting the popular, rather than strictly scientific, attention. The objections so often urged against attempts to popularize psychology seem to be growing weaker in the face of a strong demand for books of this kind. Certainly, the difficulties of presenting the science in a simple form are being much more successfully met in recent publications than they were in the earlier efforts in this direction.

As a vivid popular presentation of the subject matter of psychology, in untechnical language, and with a rich fund of illustration, Professor Thorndike's book fills very acceptably what must be recognized as an urgent general demand. The dialogue style has been adopted throughout the book. A company of persons organize themselves into an informal club for the observation and discussion of ordinary experiences. Through their own ingenuity and with a very moderate amount of assistance from books and from physicians and others in the neighborhood, they manage to cover with a good deal of completeness the topics appropriate to a text-book on psychology. Their language is very free and easy, seldom rising above purely colloquial terminology, intelligible to every reader. Their illustrations are gathered from their everyday experiences, and from the accounts which their friends gave them of similar common-place occurrences.

Much of the club's psychology will be recognized at once as orthodox from the point of view of Professor James' teachings, to which Professor Thorndike acknowledges his large indebtedness. Contri-

butions drawn from the author's own experiments on animals and on the nature of practice are also largely incorporated. When this general description of the subject matter is given, it is perhaps unnecessary to enter into any details.

Needless to say, there are conclusions reached by the club which invite criticism. But even the most critical reader of these conclusions will doubtless recognize that the work is introductory and not exhaustive, that it aims to inspire the reader to observation and study rather than to give a finished system of psychology.

There is one general question, however, which it is certainly worth while for us to consider in examining this book. What Professor James has called the "biological concept" and has definitely stated to be only a partial mode of psychological discussion has been accepted with such whole-souled assurance by Professor Thorndike that one is forced to ask whether it would not be better to treat consciousness and brain activity with more differentiation in a popular book even at the cost of introducing certain difficulties in terminology or discussion. Is it not true that a great deal of the crude psychology of the day is due to the adoption of certain physiological phrases and apparently simple explanations by those who are utterly incompetent to discuss the real relations between mental life and its physiological conditions? And is there not danger that the trained psychologist will foster this mode of thought by giving in popularized form, and without a statement of their limitations, these physiological explanations which he himself recognizes to be at best incomplete hypotheses and suggestions of what the true relations may be? Take for example one case, perhaps the most extreme case in the book before us, of direct transition from consciousness to its physiological basis. "You're quite right," says one of the members of the club on page 62, "the mind, the brain, is, of course, a tremendously complex affair, and not all of it is at work at once in any single situation." The question is not so much whether the author holds in his own psychology that mind and brain belong together in this way, as it is the question as to whether it is safe to establish in the thoughts of his general readers this easy transition from the one term to the other. For even if the transition is ultimately to be made in this way, certainly the untrained reader should be prepared for the intelligent adoption of this general attitude which is by no means the naïve attitude. If, on the other hand, the psychologist has permitted himself the phrase as a mere tentative mode of expression with the intention of discussing at some later opportunity its serious implications, or

with the intention of modifying it later for some other less easily mastered mode of expression and thought, then he should remember that in the meantime his students may be swelling the number of those who speak or write very unintelligently on psychological problems from the crudest kind of a physiological point of view. The only way to guard against the crudeness which we all deprecate in much of our popular psychology and in much of our present-day pedagogy, is to see to it that every word uttered or written by psychologists shall keep clear the distinction between mental and physiological phenomena.

Professor Thorndike's work is an admirable introduction to observational psychology because of the excellent illustrations which he has sought out and contributed in such large numbers. When it comes to popularizing the explanatory phases of the science and giving it to his audience in untechnical language, he finds, as every one is sure to find, that difficulties multiply. One could wish that this part of the work had been elaborated with more attention to the essential distinctions, even at the cost of introducing some of those technicalities which would, perhaps, have made the style less colloquial, but in the end more educative.

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Public Worship: A Study in the Psychology of Religion. JOHN P. HYLAN. (Religion of Science Library.) Chicago, Open Court Publ. Co. 1901. Pp. 94.

The decrease in church attendance, the decline of the influence of the church on the masses, the growing tendency towards Sabbath desecration, the extreme moderation of present-day theological conceptions, are conditions upon which the present volume endeavors to throw some light by a psychological discussion of the nature and meaning of public worship.

In an historical retrospect, the author finds two typical Sabbaths: (1) Recreative, held by the Jews until their captivity, viz., a day of recreation as well as rest. (2) Christian or worship conception, held by later Jews and by Christians, viz., a day for worship as well as rest. A study of the historical conditions that brought about the transition from the 'Recreative' conception to the 'Christian' reveals the psychological states involved in worship. As long as the Jews were prosperous, the 'Recreation' view prevailed, but after the captivity, when misfortunes of all kinds assailed them, a transition to the 'Christian' conception was quickly effected. Believing in Jehovah, they naturally attributed the severity of the conditions of life to His displeasure, and accordingly, in order to win again His favor, they

eagerly betook themselves to worship. In humility of spirit they throng the temples and with receptive minds receive instruction in spiritual and practical life. Thus are open immense possibilities of moral reformation by means of which the race can better adapt itself to its environment; this is the great function of the Sabbath of the 'Christian' type. In brief, while the 'Recreative' Sabbath aims simply to preserve functions already acquired, the 'Christian' conception aims at the establishment of new functions (p. 45). The essential element in producing worship, and the consequent moral reformation (adaptation) is 'adverse environment.' In emphasizing this point an interesting parallel is drawn between organic and mental evolution.

This conclusion the author confirms by his results obtained by the 'questionnaire' method (203 subjects). The answers to such questions as, "State your reasons for going to church" (personal good 173, duty 140, example 77), "Is it the music, the sermon, prayer, or something else that supplies your need in religious worship?" (sermon 52, prayer 41, music 23, fellowship 1), "Does church-going give you a better idea as to how to live?" (leads to kindness to others 30, singing 26, adoration of God 19, impulse toward a better life 6), show that the great function of religious worship is to secure the moral adjustment of the individual to his environment. These results, while extremely interesting and valuable, would be much more conclusive had they been based on a much larger number of answers than the 203 reported.

Starbuck's recent religious studies are also cited to show that conversions occur most frequently at the 'age of greatest physical growth,' when the individual is face to face with the new environment of maturity to which he has to adjust himself, and they thus support the author's general position.

This analysis of public worship enables us to understand present-day conditions. By science and invention man has been able to cope so successfully with his environment that the need of religious worship is not urgent, and so there is a very general tendency to return to the early Jewish conception of a 'Recreative' Sabbath.

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Methodology and Truth. J. E. CREIGHTON. Mind X. (1). Pp. 45–56.

The scientific account of nature embodies partly the results of observation, but partly a choice as to the way of interpreting these

results. Calling the latter the 'methodological' factor in the outcome, the question arises: how far may such a factor have distorted the image of nature presented to us? Shall we accept the whole scientific product? The writer points to the 'Synthetic Philosophy,' to 'psychology without an ego,' to ethics based on biology and overlooking 'conscious emotion and intelligent will,' as examples of failure resulting from an uncritical complaisance of this kind. Shall we reject the scientific statement altogether? Then, as alternative accounts of reality, we are reduced to offering either a 'thing-in-itself' outside of experience or an 'immediate' within it. These alone are beyond the reach of methodological disturbances, and of these the one is meaningless, the other inarticulate. Science in the meanwhile becomes mere deduction from bare hypotheses.

Now, we may reject *a priori* the view that any science, even in so far as hypothetical, is entirely without foundation in fact, for the reason that a hypothetical judgment, if meaningful, must itself be a statement of some kind of fact. 'All *a* is *b*' or 'if *a* then *b*' may not enlighten us as to the existence of *a*, but the statement has some ground for being made, and that ground is the fact recorded.

In general, a scientific formula requires critical reinterpretation before its meaning becomes clear; in proportion as it is made clear the methodological factor involved ceases to be misleading. The reflection that must guide us in the task of reinterpretation calls attention to 'the necessary abstractions which a science is compelled to make in order to get under way at all.' That is to say, a science, in order to handle a group of facts, first separates them from their context; *e. g.*, 'physical sciences consider the world *as it would be* if it existed out of relation to mind'; psychology as a 'natural science' considers 'the content of consciousness as it would exist if it were independent of any central principle of the intelligence.' 'What philosophy must seek to do is to remove these abstractions, and to evaluate the scientific conclusions from the standpoint of the concrete whole.'

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HEARING.

Zur Theorie der Combinationstöne. FELIX KRUEGER. Philos. Studien, Bd. 17 (2), 1901. Pp. 185–310.

The author compares and criticizes in this valuable paper the observations of himself and others on subjective combination tones.

Much stress is laid on the fact that the phenomena in question are for the most part independent of the existence of overtones and that, consequently, theoretical explanation must not make use of overtones unless their influence is actually proved. The author seems to attribute a little too much importance to his statement that 'Stosstöne' (a term used by Koenig) and 'Subjective Differenztöne' (used by many other writers) are not essentially different phenomena, but only different names for the same phenomena. This is no new discovery. In the index of Stumpf's *Tonpsychologie*, *e. g.*, the term Stosstöne is not found at all, but only the term Differenztöne; and the reviewer, whenever he quoted in his publications one of Koenig's observations on Stosstöne, invariably changed this name into Differenztöne. The author's observations on the relative intensity of the primary and difference tones are especially welcome, since few observers have paid to the relative intensity the attention which it theoretically deserves. Unfortunately, all estimations of the relative intensity of these sensations are still rather vague.

The second half of the paper contains a criticism of most of the more recent theories of hearing, but only of those of German writers, *viz.*, Ohm-Hensen-Helmholtz, Koenig, Voigt, Wundt, Hermann, Ebbinghaus, Meyer, Ewald, Schaefer. All these theories are rightly declared imperfect, except Schaefer's, which is called 'a good success.' Schaefer accepts the theory of resonators in the ear; he explains the subjective combination tones by an accessory theory which is based on the assumption that the inner ear is functionally comparable to a box filled with compressed air; this box has two (or more, corresponding to the number of objective tones) openings which are rhythmically opened and closed to permit the compressed air to flow out. The reviewer has never been able to see herein any analogy to the inner ear; neither has the discussion of this theory by the present author made this analogy appear clearer.

With reference to the reviewer's theory the author makes several remarks which do not seem to be quite just. I will mention here these two only: "Den Einfluss der Phasenverschiebung auf das Ergebnis seiner Curvenzerlegungen hat Meyer nicht untersucht" (p. 285). This very problem is discussed in my paper in *Zeitschrift f. Psychol.*, Bd. 16, pp. 32–33 (Figs. 9 and 10). "Selbst die Helmholtz'sche Theorie der subjectiven Combinationstöne hatte Meyer noch neben der seinigen stehen lassen" (p. 287). This seems to imply that I later declared this theory false. However, that difference and summation tones, though weak in general, are produced in the *tym-*

panum, is a mathematical consequence of its asymmetry which I have never denied. But I hold that these tones are not the *strong* difference tones heard so commonly.

It must be regretted that the exceedingly interesting and important investigations published by Emile ter Kuile in Pflüger's Archiv for 1900 are not recorded by the author. Ter Kuile's theory is fundamentally identical with the reviewer's, though different in detail. The author himself does not offer any new suggestions which might lead to progress in the physiological theory of tone perception.

Zur Theorie der Tonbeziehungen. RICHARD HOHENEMSER. Zeitschrift für Psych. u. Phys. d. Sinnesorgane, Bd. 26, 1901. Pp. 61–104.

This paper is a good exposition of the theory of Lipps, which tries to explain the psychological effects of tone combinations by assuming that a sensation of tone is *unconsciously* a series of rhythmical sensations, and by referring to the æsthetic effects of *conscious rhythm*. The author briefly rejects (and here every psychologist will agree with him) the musical theories of Helmholtz and Wundt ('overtones') and that of Riemann ('Klangvertretung'). He states that only two explanations of the musical effects are then left: Stumpf's explanation by 'fusion,' and Lipps's by 'unconscious rhythm.' He criticizes Stumpf's theory and shows that it leaves a great many important facts unexplained. Then he defends Lipps's theory against the criticism of Stumpf and applies it to a number of facts to which Stumpf's theory cannot be applied. Anything essentially new, however, is not to be found in the paper.

The reviewer confesses that he sympathizes with Lipps's theory to some extent, but is unable to understand how the musical facts can be *explained* by calling them unconscious rhythms, while admittedly the æsthetic laws of these 'rhythms' are different from those of conscious rhythms. In this respect Stumpf's criticism of this theory is as valid as ever. While Lipps's theory can hardly be regarded as an explanation, its value seems to consist in the fact that it prepares the ground for a real explanation in the future, which must be sought—most probably—in a correlation of the æsthetic effects and the chemical processes (with numerical relations) in the nervous organs. As a preparation for a future explanation Lipps's theory is doubtless more promising than that of Stumpf.

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MUSIC.

Contributions to a Psychological Theory of Music. MAX MEYER. University of Missouri Studies, I. (1), 1–80, June, 1901.

A part of this paper was published in the May number (1900) of this REVIEW; the greater part, however, is new. The first chapter contains the fundamental laws of Melody, *i. e.*, a statement of the relationships which are observable in different pairs of successive tones. The second chapter develops what I have called the 'Complete Musical Scale.' It is based on the fact that a succession of tones cannot be called a melody unless it is melodious. Since 'relationship' is only another name for melodiousness, we derive the Complete Scale by a very simple reasoning from the relationships observed in the first chapter. The third chapter contains a structural analysis of a number of well-known melodies, partly with, partly without, a primary tonic. The fourth chapter is entitled Psychological Laws Effective in the Historical Development of Melody. It is here shown that the historical development of music can easily be understood from the psychological theory. Folk song and other early music make use of the tones of the diatonic scale, which is represented in tempered intonation by the following parts of an octave: $\frac{2}{12}, \frac{2}{12}, \frac{1}{12}, \frac{2}{12}, \frac{2}{12}, \frac{2}{12}, \frac{1}{12}$. If we assume that early music prefers close relationships to less close relationships, we find that the historical importance of the diatonic scale is founded upon the fact that this scale corresponds (in pure intonation) to *two* series of tones of extremely close relationships; the diatonic scale has—as we learn incidentally—at least two (perhaps more) distinct interpretations, a fact entirely ignored by the theorists, who endeavor to reduce all music to *one* small and arbitrarily selected scale. At the same time we learn why the use of the tone 7 (the pure number 7 without a coefficient) is not found in more ancient music.

In the fifth chapter four different problems are discussed: (1) Some remarks of Charles Wead concerning the use of the number 7 in musical theory and the corresponding pitch in real music, are criticized. The fact is emphasized that this question involves, not only the pure number 7, but any product of 7 and another number. (2) Some writers have asserted that Wagnerian music could not be rendered or theoretically understood except in strictly tempered intonation. It is demonstrated that this is not true, that a sane psychological theory is able to cover Wagnerian music as well as any other kind of music, that from Wagnerian music no argument can be derived in favor of tempered intonation either in theory or practice. (3) The

fact is emphasized that violations of æsthetic laws in real art do not prove that an æsthetic theory is wrong. Many seeming violations, however, particularly in Oriental music, cease to be violations of æsthetic laws as soon as the misinterpretation by the biased European hearer is corrected. (4) It is shown that different parts of the same piece of music are sometimes constructed according to different æsthetic laws. Therefore, in order to understand the structure of a complicated piece, all the parts must be studied singly.

The sixth chapter begins with a discussion of the general æsthetic laws of harmony. In harmony the same relationships are found as in melody, *i. e.*, relationship is effective in simultaneous tones as well as in successive tones. Besides relationship, we observe in harmony *consonance*. The failure to distinguish between these *two* contents of harmony is a serious fault of the musical theories. Consonance is psychologically effective without analysis, but relationships are effective only so far as a sound is heard analyzed. This leads to a brief consideration of the theory of "quality of tone" of a compound sound, which is not satisfactorily treated in the psychological text-books. Stumpf's theory that quality is observable in simple as well as in compound sounds is accepted. Several typical forms of music are then harmonically analyzed: (1) The drone bass of bagpipe music. (2) A mediæval canon. (3) A polyphonic piece by Heinrich Schuetz. The analysis of this piece is especially important because it explains the often-observed falling of pitch in the performance of polyphonic vocal music. (4) A song of Schubert's with its accompaniment. (5) Several measures of the duet in Wagner's Tristan and Isolde are analyzed. This analysis demonstrates that no reason exists for assuming that Wagner's music is based on any æsthetic laws different from those of other music. (6) The æsthetic effect of the 'leading chord' is explained by the psychological theory without the aid of any accessory hypothesis. THE AUTHOR.

EXPERIMENTAL.

Studies from the Psychological Laboratory of the University of Michigan. Contributed by W. B. PILLSBURY. Amer. Jour. of Psychology, 1901, XII. (2).

I. *The Fluctuations of the Attention in some of their Psychological Relations.* J. W. SLAUGHTER.

II. *The Effect of certain Stimuli upon the Attention Wave.* R. W. TAYLOR.

Masson's disc was used and the fluctuations were registered upon a moked drum. A normal series showed, in addition to the quickly

alternating periods in which the stimulus was visible and invisible, two longer fluctuations, one from 10 to 15 seconds, and the other from 60 to 80 seconds in length. A dynamograph was introduced to try the effect of an accompanying physical exertion. This made the periods of visibility and non-visibility shorter with a relative increase in the length of the former, and thus had a similar effect to an increase in the intensity of the stimulus or the increase of attention. A finger-plethysmograph was employed to discover what relation might exist between the fluctuations and blood pressure, and it was found that with most subjects they ran parallel with the Traube-Hering waves, while with some the breathing exerted the controlling influence.

With similar apparatus Taylor tried the effect of agreeable and disagreeable distracting stimuli upon the rapidity of fluctuation. With one subject pain from an induction current quickened the rhythm of fluctuation with relatively increased length of the invisible periods, while with another the effect was exactly opposite. For one subject smoking lengthened the waves and also the relative length of the visible periods.

The work is preceded by an interesting historical résumé, and the experiments suggest important physiological conditions of the phenomena. The methods, however, are not suited to the complicated conditions, and the work suffers from the lack of critical analysis of the problem. Subjective fluctuations should have been differentiated from the others, and means should have been employed to indicate involuntary eye movements.

J. P. Hylan.

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Ueber Arbeit und Ruhe. Ernest H. Lindley. Psychologische Arbeiten, III., Heft 3. Pp. 482–534.

This paper of Professor Lindley is one of the most interesting of the recent products of our psychological laboratories. It owes its value essentially to the skill with which has been used an excellent method developed by Kraepelin and those who have worked with him. To the psychologist not yet acquainted with the work of the Heidelberg alienist the dominant interest of the paper will not be the particular results it sets forth, although they are of unusual weight, but the method itself with the unexpected precision of its revelations.

In continuation of the researches of Amberg, Rivers and Kraepelin, Professor Lindley addressed himself to the task of discovering the influence of periods of rest of various lengths upon mental work which they interrupt. No more than what is absolutely necessary

for a general understanding of the investigation before us shall be said here concerning the method and the often complicated calculations by which the conclusions we are about to report were reached.

The experiments extended over 28 days. Two of them were days of rest. On the other days 60 minutes were spent by three persons in adding columns of printed figures. The number of figures added was taken as the measure of the amount of work done. On the first day the 60^{m} were continuous. On the second day there was a rest of 5^{m} after the first half hour. On the third day the rest was of 15^{m}; on the fourth of 30^{m} and on the fifth of an hour. This series of five days was repeated five times. The figures obtained show several variations relatively regular for each subject. They are interpeted as due to the following factors, to each one of which a section of the paper is devoted: 'Uebungsfähigkeit,' 'Uebungsfestigkeit,' 'Ermüdbarkeit,' 'Anregbarkeit' and 'Antrieb.'

It is when considered from the point of view of comparative individual psychology that the most interesting aspect of Professor Lindley's work is seen. Each one of the three subjects yields results strikingly different from those of the two others and yet the regularity and the consistency of the figures are sufficient proof of the reliability of the experiments. Let us consider, for instance, the capacity for improvement (Uebungsfähigkeit), as it appears in the following table:

	1	2	3	4	5
A	2118	2781	2891	2967	3065
B	1619	1939	2366	2649	2812
C	1486	1655	1768	1795	1781

A, *B*, and *C* denote the three subjects. 1, 2, 3, 4 and 5, at the head of the columns, indicate the sets of 5 days in their chronological order. The numbers in the columns give the average daily number of figures added during each set of five days. (The average of the second set for *B* is from four days only.) It appears from this table that *C* was initially a great deal slower than *A*. For this initial difference the experimenters knew not how to account. The subjects differ also widely in their improvement-capacity. If the gains are expressed in per cent. of the number of figures added for the first day, the following figures are obtained:

A	10.	0.7	1.1
B	12.2	2.6	1.9
C	3.6	0.8	0.6

The first column is based upon the average of the first ten days; the second upon the average of the second 10 days and the third column upon the average of the last 7 days. *A* began with the largest number of additions. Her gains were also at first absolutely the greatest, but *B* gained from the start proportionally more than *A*. That is, *B* shows the greatest and *C* the smallest gain-capacity of the three.

If one calculates the losses suffered from day to day on account of 'Uebungsverlust'—loss of practice-gain—it is found that the advantage rests again with *B*, inasmuch as he loses only 38% of his practice-gain, while *A* loses 74%. *C* stands between them. In other words, in the case of *A* the greatest absolute gain is followed by the greatest loss, while in *B* we have the greatest capacity for improvement—Uebungsfähigkeit—associated with the greatest gain or retentiveness—Uebungsfestigkeit.

If we now pass to fatigue we find that *C* is susceptible to it in an astonishingly high degree. His fatigue coefficient is 13%, that of *A* 3.8%, and that of *B* 1.8%. These fatigue-differences appear clearly in the effect of the pauses introduced between the two half hours of adding. The following figures give in per cent. of the number of figures added during the first half hour the gains made during the second half hour. The first column is an average of the five days when the two half hours were not interrupted by any rest; the second column the average of the days in which a pause of five minutes came between the two half hours and so on:

A	+ 1.8;	+ 0.8;	+ 4.8;	+ 4.6;	+ 3.5
B	+ 4.2;	+ 1.2;	+ 2.7;	+ 3.1;	+ 2.5
C	− 11.3;	− 7 ;	− 6.4;	− 3.9;	− 0.5

In the case of *C*, the second half hour is always worse than the first, whatever be the intervening rest, but it is least so when the rest interval is the greatest, *i. e.*, 60^{m}. A longer rest would apparently have been of still greater advantage to him. *B* did best during the second half hour when there was no interruption between it and the first, and worst with the smallest rest, 5^{m}. In the case of *A*, the most favorable rest period under the circumstances was 15^{m}. No one will fail to see the bearing of these differences upon the problem of school fatigue.

It is not to be supposed that the results following upon the rest-interruptions are to be ascribed solely to fatigue. Other factors come into play, notably the tendency to lose what has just been gained by practice, the 'Anregung' and the 'Antrieb.' Rests of 5, of 10, or of

15" resulted in a loss for *B*, because they were long enough to destroy the 'Anregung' and to cause a sensible loss of practice, while the compensation due to rest was apparently insignificant, either because there was very little fatigue or because the time allowed was not sufficient to relieve it, or for both reasons.

We cannot speak in detail of the chapters on 'Anregung' and 'Antrieb.' The more general conclusion reached in the latter is that 'Antrieb' is independent both of the capacity for improvement and of fatigue.

In the chapter on 'Personal Differences' attention is drawn to the light thrown by this and similar investigations upon the human personality, and the results obtained by the author are compared, as far as possible, with those of Hoch, Amberg and Rivers.

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ASSOCIATION.

Zur qualitativen Untersuchung der Association. A. MAYER und J. ORTH. Zeitschrift f. Psychol. u. Physiol., Bd. 26, Heft 1 u. 2, Mai, 1901.

Experimentelle Untersuchungen über Associationen. G. CORDES. Philosophische Studien, Bd. XVII., 1 Heft, März, 1901.

Herren Mayer and Orth discuss those associations 'in which the subject reacts to a word spoken to him by a spoken word.' The report of their experiments is disfigured by the constant use of the term 'association' in an inaccurate sense, as applying only to the word pronounced by the subjects in response to the stimulus. The intermediate conscious phenomena are described as 'eingeschaltete Bewusstseinsvorgänge,' whereas these are, of course, themselves associations and, on the other hand, the word, as *pronounced*, is, strictly speaking, a physical, not a psychical phenomenon. The main conclusions of the investigation are the following:

1. Between the verbal stimuli and the verbal responses, in these experiments, there commonly occur intervening conscious phenomena; and such reactions are longer than those in which the verbal response follows without preceding mental phenomena.

2. Feelings and volitional experiences may be members of this psychic series between the experimenter's and the subject's word.

There is certainly nothing new in all this, and the simplicity and roughness of the method, especially of the time-measurement with the observer's stop-watch, hardly justifies the detail of the report. The

results of the experiments certainly do not lead to any new classification of association, and the forms of 'Eintheilung' proposed by the authors are based on no fundamental principle whatever. Their conclusion, that disagreeably toned 'intermediate ideas' lengthen the association more than agreeably toned ideas, will hardly be accepted without confirmation by experiments far more rigorously conducted.

The praiseworthy feature of the work of Mayer and Orth is the stress which they lay on the need of careful introspection on the part of subjects in association experiments, and on the necessity of a careful study of these introspective results. This emphasis on the value of introspective records is the distinguishing merit, also, of the far more significant work of Cordes. In the main, Cordes follows Scripture's method, even making use of the mammoth black box of Scripture's investigation (*Philos. Stud.* VII.). The subject of these experiments sits inside the black box and receives the stimuli, which are mainly visual. The recorded associations are absolutely free, that is, the subject is directed simply to tell in detail what images or feelings or other experiences have followed upon the occurrence of the stimulus. One must read the report of Cordes itself in order to realize the discrimination, caution and skill of the analysis and classification of this material, gained by experiment during several months with eight subjects. The extreme care with which Cordes has studied his associations gives great weight to his two negative conclusions. He insists, in the first place, on the futility of all classifications of association depending on the relation of a word as pronounced to a word as heard. This follows, first, from the artificiality of all these verbal associations, and second, from the marked individuality and the illimitable variety of associative series. In the second place, Cordes asserts in unambiguous terms the impossibility of measuring association times. This conclusion is based on the fact that in all complex and affectively toned associations the verbal expression is inevitably retarded.

This brief notice of the work of Cordes does no justice to his discriminating criticism of Ziehen and Aschaffenburg, nor to his interesting experiments in mediate associations, which he finds as result of experiments with repeated nonsense syllables. (S. 71 *seq.*) To the writer of this notice, the value of the work lies, however, far less in its experimental conclusions than in the strictly psychological nature of the method and of the point of view. It surely is significant of the emancipation of psychology from physics that this latest output of the Leipzig laboratory should assert the impossibility of

measuring association times! One hopes that the later work of Cordes will evidence a more cosmopolitan acquaintance with the literature of his subject. He does not allude to any one of the recent investigations and studies, in English, of association.

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CHILD AND ANIMAL PSYCHOLOGY.

The Study of Children. EDWARD THORNDIKE. Teachers College Record, edited by James E. Russell. Columbia University Press, N. Y., May, 1901. Pp. 110.

The general character of this pamphlet is stated in the preface as follows: "The purpose of this number of the *Record* is to give an account of the work done by the department of Psychology in the Teachers College and to present some of the more important data of child study in a form accessible to all students of children and convenient for teachers of special subjects. It will comprise brief statements of the nature and aims of the different courses offered, a more detailed account of the elementary course in child study, reprints of two valuable general articles now practically inaccessible to most students, and sample contributions from the literature of child study to the methods of teaching special subjects."

Three courses in child study are offered, one dealing with the more practical phases of the subject, one with theories of genetic psychology, and one with original researches in genetic and comparative psychology.

The work in the first course is outlined and its character illustrated by samples of questions asked in connection with the critical study of the literature of child study, while the work in the other courses is more briefly outlined.

The study of the contributions of child psychology to special methods is along the lines of reading and literature, writing and drawing, and spelling. Considerable portions of articles by Wissler, Bryan, Hancock, and Rice describing their investigations are quoted with comments and suggestions as to the value of methods used, the accuracy of generalizations made, and their practical application in education.

The child study reprints appended are portions of Johnson's 'Rudimentary Society among Boys,' Schallenberger's 'A Study of Children's Rights, as seen by Themselves,' and all of Galton's 'History of Twins.'

Naturally, in this as in other early attempts at organizing the materials of a new subject of study there is some lack of unity and symmetry, yet every one interested in the teaching or study of child study will find this pamphlet very suggestive and valuable both as an outline and because of the details given.

Some Criminal Tendencies of Boyhood: A Study in Adolescence. EDGAR JAMES SWIFT. Pedagogical Seminary, March, 1901. Pp. 27.

This is another of the many *questionnaire* studies published in the Pedagogical Seminary. Like most of the others it is valuable chiefly as a preliminary examination of complex phenomena. Just as a view of a country from a balloon would be of some value to a surveyor, so such 'drag-net *questionnaires*' are suggestive and helpful preliminaries to scientific investigations of new questions.

The object of the *questionnaire* was to determine whether men of unquestioned moral character ever performed or felt impulses to perform acts of a criminal nature, such as have caused boys to be sent to reform schools and men to prison. No tables are given, but it was found that a large proportion of the forty-three teachers, twenty-five college and normal students, and thirty-five business and professional men who answered had felt such tendencies and in many cases had actually engaged in law-breaking and truancy, attempted violence and even murder in fits of anger, robbed orchards, etc., stolen goods and money from relatives, employers and others, set fires and destroyed property in various escapades, and often lied to escape punishment. Considerable space is given to quotations from writers on sociology showing the customs of savage people that are comparable to the criminal impulses and acts of adolescent boys of civilized parents and the general conclusions of the study are indicated by the following quotations:

"The race tendencies of primitive man cannot properly be classified under morality. They are neither moral nor immoral. They are simply stages in evolution in which man finds himself and to the conditions of which all his nature strives to conform."

"Adults read their ideas of morality into children's acts and then catalogue them as right or wrong, while in reality they are often neither. This period of savagery or semi-criminality is normal for all healthy boys. Those whose surroundings are favorable to a life of crime continue in it, finally to end in the reform school, and still later, probably, in the penitentiary, while those of better sur-

roundings pass through it without permanent moral injury, and perhaps, indeed, with a stronger character and a keener insight into human nature."

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The Child. A Study in the Evolution of Man. A. F. CHAMBERLAIN. London, W. Scott, N. Y., Scribners (imported), 1900.

Dr. Chamberlain gives us in this work one of the best summaries of child study yet published. Certain chapters of child life are selected for treatment; historical sketches referring to the chief advances achieved in the knowledge of the subject are given, and an eminently fair, critical attitude maintained throughout. The extensive reading of the author is apparent in every chapter. The bibliography is eminently useful; in fact, the whole book gathers together a fund of information and sources valuable in the class-room and readable for all interested in the knowledge of the child.

No criticism is offered on the book, but several points concerning the doctrines treated should be noted. For instance, in reference to the so-called prolongation of the helplessness of infancy, it should be observed that the essence of the problem consists in the growth and increase in plasticity of the nervous system. This increasing plasticity allows greater adaptability and the acquisition of an ever-increasing social heritage. It is increase of plasticity rather than prolongation of *infancy*. Moreover, the progressive medullation of nerve fibers will determine the real limit of this age of plasticity. Thus adolescence really extends to the age of 35 and 40, according to the investigations of Kaiser, Kaes and others. The final justification of the increase in plasticity is, of course, the growth of adaptability.

Chamberlain seems to accept the argument of Fiske's that the prolonged helplessness of the offspring must keep the parents together for longer and longer periods, and that out of this grew sociality, the family, the clan, society. Out of the helplessness of the infant grew the helpfulness of man. As a matter of fact it was due to parental care that plasticity had a chance to develop. Without parental care all variations in the young tending to greater plasticity would increase the helplessness of the young, and hence diminish their chances for survival. Only when such variations as tended to increase the helpfulness of the parents occurred was there a possibility for plastic variations in the young to be sheltered until their growth rendered them useful. The growth in plasticity was the goal, and the helpfulness or

sociality of family, clan, etc., was the means. This is, of course, important from the standpoint of social psychology.

In reference to play it may be stated that Herbert Spencer emphasized the instinctive origin of play ('Principles of Psychology,' Vol. II., chapter on Æsthetic Sentiments). The present polemic against the surplus-energy theory will have to be modified somewhat to be even with the facts. The main characteristic of play from the social and biological standpoint is its *initiatory* character; social habits are to be formed which later may be transferred or attached to other aims and ends than pigskins, tops and prizes. Gambling as a play is condemned because of its anti-social habit—waiting for something to turn up. Groos's theory implies that the young are possessed of impulses (broken-down instincts owing to increase of plasticity?) and that play offers an opportunity for the development of these impulses into instincts useful to the adult. Hence the *Vorübung*, *Ausübung* and *Einübung*. In what respect will the adult be better off? Why should not natural selection prefer the full-grown instincts from birth on? To state that play favors the growth of intelligence is to state a fact; it is also the problem of play.

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Experimental Study of the Mental Processes of the Rat. II. WILLARD S. SMALL. American Journal of Psychology, XII. (2), January, 1901.

In 34 pages Mr. Small presents the results of his study of the behavior of *Mus decumanus* in learning to go directly to the center of a complicated maze. The paper completes his studies of the mental processes of the rat, previous sections of which have been already noted in the REVIEW. Learning a complicated path to a desirable place had been found a useful test of mental traits in the case of chicks and fish and served a good purpose in Mr. Small's hands. He perhaps lessened the value of his observations by leaving the rats to roam about the maze unobserved all night after each trial. It is hard to interpret behavior surely if we see only broken abstracts instead of the continuous whole.

Mr. Small presents evidence of the superior energy and consequent quickness in learning of the males, of the absence of imitation, of the permanence of instincts in spite of long disuse, of the importance of motor and tactile consciousness in the rat's mental economy, of the gradual progress of the animals' learning, and of the lack of reasoning in the common psychological use of the term. His comments on

general animal psychology are interesting and sagacious, though they seem to the reviewer to be concerned too much with verbal rather than real questions. I may be allowed to correct a mistake concerning the 'curves' or graphic representations of the progress of animals in successive trials which I have given in two monographs on animal intelligence. Mr. Small and others credit me with the opinion that these have some significance beyond their face value, as handy graphic representations of the decrease in time taken in successive trials. That is not the case. I made use of them first of all to save space, and secondly, because I found empirically that with cats, dogs, chicks and fish the decrease in time was fairly proportionate to the increase in the tendency to select the appropriate act, and so served as a fairly accurate representation of it. In the case of the monkeys I expressly limited the meaning of the time records.

Mr. Small insists that the rats possessed and were guided by images and feelings of recognition, and puts his descriptions of their learning constantly in terms of images, conscious selection, doubt, etc., with never a word about the impulsive side of animal consciousness. This seems unwise. An animal may feel like going down a certain path, or feel like hesitating, or feel like doing one thing where he could do either of two or more, and still have no images or ideational consciousness whatever. So may human beings. Mr. Small has not, I think, realized the possibilities of learning by the selection of impulses.

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ÆSTHETICS.

La valeur sociale de l'art. G. SOREL. Revue de Métaphysique et de Morale, IX., 3, May, 1901. Pp. 251–278.

The problem of explaining the genesis of a work of art in the mind of the artist is, M. Sorel believes, an impossible task. A psychology of genius—a science of the accidental—would be a condition of such a task. But the production of the work of art in so far as a given society is concerned, the determination of the reasons why such a work is so received, seems to him a less formidable undertaking. What then, is art in its relation to society? Is it, as many believe, merely a source of special pleasures? The imperfect state of the psychology of the feelings makes a decisive answer impossible. Art criticism has been too superficial to furnish what might be valuable psychological data. Is the meaning and value of art to be determined by its moral effect?

The moral effect is of very great importance, but it can seldom be determined directly. Time alone can show what the effects of a work of art will be.

The history of art may give some clue. Ancient art differed from modern art in many respects, but the most striking difference, M. Sorel believes, was that ancient art combined the useful and the æsthetic, *e. g.*, the architecture of temples and of tombs, religious rites, etc.

As the arts developed, technique was perfected and finally became all-important; professional skill took the first rank. Art became abstract and sought its justification in the metaphysical theory which made *beauty* the aim of art. But these schools were gradually broken up by a growing individualism which has allowed the dogma of beauty to disappear.

This individualism is charged with merely aiming to please by whatever means; with being regardless of morals or even antagonistic to them. Is art, then, merely a dangerous amusement? The theory that leads art back to play has been developed most completely in England. M. Sorel believes it to be the outcome of economic conditions, in particular the presence of a rich and powerful aristocracy and the presence of superfluous energy. This energy must be expended and art seemed to be its noblest object. In the Renaissance, too, the conception that art was for the pleasure of the aristocracy was widely current.

The play element and the pedagogic element cannot, perhaps, be eliminated, but art can no longer exist for privileged classes. It must become an art of the people, like the Greek art with its sacred dances, choruses of singers, public fêtes, etc. Modern music suggests possibilities in that direction. Moreover, art must be brought into relation to modern ideals. It must find its place in a society which regards work as the most important thing in the world and is becoming more and more intensely absorbed in work. The art of luxury and play must become an art of relaxation—a relaxation which is necessary for intellectual sanity.

The most interesting side of modern art is that which unites use and beauty, which shows the mind of the worker in the work of his hands—not an art for the collection, but an art for use. It should have regard for the means of production rather than the things produced—for the perfection of the whole, and of each of the parts with reference to their place in a useful whole. Such an art, expressing the mind of the worker, will be individualistic. Æsthetics will become more and more intellectual, since art *is* the expression of mind

in work. He who does not understand a given art may understand its æsthetic value by reflection on the interpretation of professionals and by the analogies that exist among all the operations of the inventive mind.

Art should ennoble work and raise manual labor to the level of scientific work.

An obvious criticism on M. Sorel's whole proceeding is his reliance on an interpretation of contemporary sociological and economic conditions for the basis of a scientific theory after rejecting certain psychological interpretations on the ground that they were insufficiently established. His discussions of the economic conditions responsible for the 'play theory' of art do not show clearly in what way superfluous energy is related to art production. His interpretation of Greek art fails (it would seem) to apply to a large part of Greek art. Again, his conclusion as to the growing intellectualistic character of æsthetics seems somewhat hasty, if indeed it is not a begging of the whole question.

His whole article, however, is very interesting; in particular his discussions relating to the ethical bearings of art and the relations of art and work. In the latter he is not far from William Morris's point of view: "An art by the people and for the people, as a joy to the maker and user."

The article contains incidentally criticisms of Taine, Guyau, Brunetière, Prudhon, and Tolstoi.

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Das Problem des Tragischen. RICHARD HAMANN. Zeitschrift für Philosophie und philosophische Kritik, Bd. 117, Heft 2, and Bd. 118, Heft 1.

This treatment of the problems of the tragic revolves about a distinction between the purely tragic as a subjective value and the objective, æsthetic treatment of the tragic in the tragedy—a distinction which in the history of æsthetic discussion has scarcely been made so marked before, and which has for its object the attainment of a definition which may include the profound modifications made by our modern life in the conception of both.

His investigation of the tragic moment as such, independent of its expression in the drama, leads the author to a position at variance with emotional and ethical theories of Katharsis, as they have found expression in classical criticism. The emotions of fear and pity alone do not suffice to explain the tragical moment. As the later theories of

the tragic have all maintained, in order that these emotions may be tragic they must proceed from the destruction of an ethical person or value. An ethical norm of value, be it ever so shifting, must be presupposed. Hamann finds himself equally at odds, however, with the classical ethical doctrine of the tragic as it finds expression in Lessing, Schiller, and even in a modified form in Lipps. Through all its variations it conceives of the tragic as tragical elevation, and this as brought about by an element of rational reconciliation following upon an ethical dissonance. The two concepts of tragical guilt and poetic justice, while emphasizing the ethical norm or value as a presupposition of the tragic, err in conceiving as a part of the tragic moment an element which would simply destroy the tragic, substitute for it another value. The tragic remains then, ethically, a dissonance resulting from the negation or destruction of an intensified ethical value or norm. "Von Tragik sprechen wir dort wo wir die Zerstörung eines Selbstwerthes, dem wir sonst Existenzberechtigung zuschreiben als ein ungerechtfertiges Verhängniss, einen Widerspruch mit unserer ethischer Norm, von dem, was sein soll, empfinden. Die Tragödie ist davon nur ein besonders ausgezeichneter Fall."

For Hamann, the conception of reconciliation included in the tragic does nothing more nor less than rob it of its tragical character. It is nothing but a 'displacement' of the norm of value, a robbing of Peter to pay Paul. A value is intensified only to act as a foil which shall increase the value of the ethical norm which displaces it; it may be a vision of an inviolable world-order of the will of God or of mere Fate. The author would probably find here a more complex sentiment —sublimity, tragical elevation—but not pure tragic. The tragic as such remains ethically a dissonance.

But tragedy, as an art, is but a special case of the tragic. And in the dramatically tragic is undoubtedly to be found, in place of dissonance, consonance, katharsis, elevation. Whence this new modifier of the tragic? "Wie kann das was in seiner ethischen Konsequenz abstossend wirkt, æsthetisch werthvoll werden?" The answer to this is wholly in terms of the conditions of the æsthetic psychosis. The reconciliation and its katharsis, in so far as there is one, is, from the ethical point of view, *illusion*, as long as the tragic remains tragic. The consonance is wholly one of function of our sensuous, our emotional and judgmental processes. The ethical dissonance remains in suspension. The technique of the drama is especially favorable to this twofold state of consciousness. The quick procession of pictures, the piling up of intense sensuous, personal and ethical

values, the rhythm of emotions, hope and fear, pity and hatred, the semi-logical satisfaction from the movement of the plot—all produce equally an intensification of the value of the personality involved, and a consonance in the merely intuitive processes. Even ethical values in the presentation become simply elements to enhance the value of the æsthetic whole. Though Hamann nowhere indicates as much, the ideas of tragical guilt, of fate, of poetic justice, which appear so constantly in the classical drama and have often been conceived as essentials of the tragic, would presumably be interpreted by him simply as means among others to produce the æsthetic consonance and necessity of the drama, at a time when they were profound beliefs, and not an essential part of the ethically tragic.

The undoubted basis of this fundamental distinction between the tragic and tragedy is an ultimate distinction between the ethical and the æsthetic. As a provisional differentiation this is important, and the distinction between the tragic and tragedy is, therefore, valuable provisionally. It has also to recommend it, the ease with which much of modern tragedy is explicable under this formula. The reviewer does not feel, however, that Hamann has done justice to the ethical meaning of tragedy, meaning by tragedy the union of the ethically tragic and the æsthetic psychosis. 'Tragical elevation' is itself an ethical sentiment and of ethical significance. The healing of the breach would follow only from a study of the ethical meaning of the æsthetic experience itself. This, of course, was not the writer's problem, and his provisional distinction is enlightening.

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PERCEPTION OF TIME.

Perception of Change and Duration. G. F. STOUT. Mind, IX., 1–7, January, 1900.

This is the presidential address delivered before the Aristotelian Society, November, 1899. It discusses the question as to how far and in what sense it is necessary, when we perceive a temporal process, that representations of prior parts of the time series should be present to consciousness in the perception of succeeding parts. Green and Hodgson are cited in support of the view that in perceiving a time sequence the presentations of prior stages must persist in later stages. This may be called the memory-image theory.

The views of two recent German writers are stated and criticized. Meinong defends the memory-image theory, but on theoretical rather

than on introspective grounds. He maintains, for example, that a melody is not presented until the last note of it is heard. Mr. Stout admits that "we must have *some sort* of apprehension of the terms related in order to apprehend a relation," but denies that these terms must necessarily be presented as distinct memory-images.

Schumann, on the other hand, holds that for psychical elements to form a whole they need not be presented together. The accompanying process which makes such perception possible need not be a memory-image; it is sufficient to assume a physiological or psychical disposition. As Mr. Stout puts it in his *Manual of Psychology*, "The persistent traces of past experience may modify present experience and be modified by it, without reappearance of the content of the past experience in the actual moment of present consciousness. * * * Successive series of a rhythmic character can at their close be apprehended as a whole without mentally reproducing and discriminating in the moment of apprehension the several sequent parts which compose them" (pp. 78–79).

The clue to Mr. Stout's own view seems to be found in the following sentence: "To 'form a single whole' means to act as a whole." This accords with his teaching also in his Psychology. It is because a given succession of sounds, for example, stimulates an incipient response on the part of the organism that this series of sounds is apprehended as a whole. This incipient act of the organism requires such a series for the maintenance of its conative unity and continuity, hence this is the point of view from which to interpret the perceptual process. The time unit is always ultimately the activity, as represented in the interest or end. This explains why "a man may be able to estimate lapse of time with a fair amount of accuracy, and yet not have established an accurate relation between his subjective estimate and time as measured by the clock" (p. 387).

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APHASIA.

Weiterer Beitrag zur Kenntniss der Beziehungen zwischen Aphasie und Geisteskrankheit. Privatdocent Dr. Karl Heilbronner, Halle a. S. Zeitschrift f. Psychologie, Bd. XXIV., 83.

The present article is a continuation of previously published examinations of an individual case (Wernicke's Psychiat. Abh., Heft. I., and Zeitschrift f. Psychiatrie, Bd. LV.).

The patient was a man who, in consequence of an acute mental disturbance, with hallucinations, etc., superficially giving symptoms

of dementia, showed when studied more closely that his defects were referable in large part to the portion of the brain connected with speech. These showed themselves as a motor and sensory aphasia, and in disturbances of the higher intellectual functions which are most closely allied to speech. Thus the apparent dementia was elective, as in many respects the patient in the course of time became as intelligent as he ever had been. From this Heilbronner infers that the pathological process which the psychosis caused, affected most intensively the brain regions in the neighborhood of the speech centers. Though in these regions the damage done was irreparable, thus giving a permanent picture of aphasia, in other parts of the brain recovery had taken place permitting a certain degree of restoration of the intelligence.

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NEUROLOGY.

A Study of the Neurofibrils in the Ganglion Cells of the Cerebral Cortex. STEWART PATON. Journal of Experimental Medicine, Vol. V., No. I., October 1, 1900. Pp. 21–25. One plate.

The illustrated article of which this abstract is a brief report and summary adds good evidence to the much-disputed question (still unsettled?) as to the existence or not of fibrils within the cell-bodies of cortical neurones. Dr. Paton has no doubt that these fibrils do exist and his illustrations of them make their appearance very clear.

He suggests that the various elements of the nerve-cell body would be classified into four groups better than by the classification common at present, although these would be general rather than strict. These classes are: '(1) cytoplasm; (2) Nissl bodies; (3) fibrils; (4) constituents whose morphological characteristics are not yet definitely known.' This nomenclature would be an improvement especially because it would do away with the very objectionable names in use at present, based on the relative stainability of the structures in question.

It appears certain to the author that the fibrils run directly through the body of the cell; and he thinks that the reticulum connecting these fibrils is outside the cell rather than within the boundary—the latter being relatively a novel view of this largely examined fabric. The 'gemmules' or varicosities (which some still consider artefacts), this observer regards as organs, of some use or other, closely related to the fibrils. (See abstract of article by Stefanowska on page 653 below.)

The fibrils, it appears to Dr. Paton, can be properly stained only by the hematin or the gold stains used by Apáthy or by a method of Bethe's as yet unpublished. A hematin method altered by the author is presented, but this even is admitted to be not entirely satisfactory. Thus it is clear that the staining of the fibrils is a process of great difficulty leading to no very good results, although the author considers that the ordinary lack of freshness of the tissue-material, and especially of human cortical material, is to blame for a great part of this defect in definiteness. The fibrils decompose *very* soon after death, and human material dead less than a day or two is very rarely obtainable. Perhaps to this important consideration is due much of the scepticism regarding these fibrils still preserved by a few experts in neuro-histology. This is a presumption indeed not difficult to accept in view both of the natural probability and of the exact evidence offered by highly competent observers.

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A Digest and Criticism of the Data upon which is based the Theory of the Ameboid Movements of the Neurone. H. H. BAWDEN. Journal of Comparative Neurology, X. (2), May, 1900. Pp. 243–264.

The title of this article very fully expresses the latter's nature, except that it fails to indicate that a bibliography containing 115 references to articles and works on this interesting subject may be found at the end of the paper. Taken altogether the article is one of uncommon value to the psychologist, for, in a brief way, it summarizes for him the facts, theories and opinions of this almost uniquely interesting supposition—that the neural units are primarily active, as well as passive, agents in the transmission of portions of the psychophysiological process. But besides this it adds yet another, and a highly competent, opinion as to the respective values of the researches digested, and as to the probable status of the whole matter.

Time is not available for the summarizing of the historical portion of the paper; it is very accessible, but full of the details of a highly technical histology and microscopy, interesting to few save the specialists in these matters. Our brief summary will be confined to the critical portion of the article.

Following Duval and Deyber, Dr. Bawden divides the total presented evidence for the theory into two classes, first, that derived from analogy, and second, direct 'proofs.'

The former class of arguments is based almost wholly on the ameboid movements of the cells of either epithelial, glandular, or cicatricial tissue or on those of the more recently described myo-epithelial cells of the sweat-glands. In criticism the author points out that these movements so far as epithelial and gland cells are concerned take place within the bodies of the cells and are not at all concerned with any alteration of their external contours. Ameboid movement in cicatricial tissue-cells, that is chiefly in case of the leucocytes, no one pretends to deny, but the resemblance between these (the interesting and mysterious white corpuscles of the blood and lymph) and nerve cells is by no means apparent; they do not seem properly comparable in the direction concerned. Again, the waxing and waning of pigment in retinal cells, given by some as analogical evidence, is likewise not at all comparable to the supposed neuronal movements in question, for the granules of pigment merely appear and disappear functionally within the bodies of the retinal cells—the cells' outlines are not changed thereby.

The direct evidence offered by the supporters of the retraction theory relates mainly to the structural and functional status of the gemmules or varicosities seen by a number of observers on the neuronal projections of different kinds. Demoor, Stefanowska and Manouélian furnished Deyber and Duval their chief observational data [see an abstract of a report of work by Stefanowska on this subject, following].

The "facts show," says Dr. Bawden, "that a theory of movements of the protoplasm of the neurones cannot be carried out without constantly interpreting the histological evidence in terms of the histological procedure." This is apparently the root of the whole matter, for it seems that 'histological procedure' produces certain changes in protoplasm sometimes scarcely to be distinguished from natural organs. In fine, then, the critic considers many of the appearances noticed by a number of good histologists to be nothing more than artefacts produced by manipulation; or else that too little allowance has been made for the changes (other than protoplasmic movements in the sense obtaining in this case) which occur in cells under varying physiological conditions. "It is not denied that the neurones may possess an ameboid property. It is simply denied that the evidence which has confidently been put forward in support of the theory is wholly trustworthy as it has been employed. The real problem still remains, which is to demonstrate the existence of this property in living [neural?] tissue. It must be shown in some way which will obviate

the effects of the reagents used, that there is an actual change in the spatial relation of the ultimate ramifications of the nerve cells."

The opinion then derived from a careful study of all available material, which Dr. Bawden wishes to express in this widely summarizing paper of his, is that the theory of ameboid movement of the neurones is unsupported by the facts. And, after all, it does seem probable that the whole hypothesis and the great mass of work and discussion it has occasioned is a rather too striking example of science by the deductive method—that the wish is father to the supposition.

Sur le mode de formation des varicosités dans les prolongements des cellules nerveuses. MICHILINE STEFANOWSKA. Annales de la Societé royale des sciences medicales et naturelles de Bruxelles, Tome IX., fasc. 3, 1900. Pp. 1–18.

This article is a report of an enlightening research done at the Institut Solvay, the details of which the specialist may find in the third part of volume third of the *Travaux de l'Institut Solvay*, under the title, 'Localisation des altérations cérébrales produites par l'éther.' In this present paper, however, the proper product of the two years' work is reported and in a form valuably suggestive to the physio-psychologist. Eight illustrations add to the clearness and conciseness of the article.

The varicosities in question, it is perhaps needless to explain, are those minute nodules or enlargements which, seen so often on the various branches of neurones, have caused so much discussion and conjecture. One of their chief causes of interest to the psychologist lies in their quasi-relation to the now perhaps decadent hypothesis that the ends of the neuronal prolongations spontaneously extend and retract in performing their various functions.

Mlle. Stefanowska's general method of studying these varicosities was simply to etherize white mice to various degrees, examining then their cerebral cortices, properly prepared, under the microscope with high-power lenses. As has been said, she devoted two years to systematically carrying out this, in principle, simple procedure, making it probable that her opinions on the matter are worthy of more than passing notice.

The researcher considers that the changes taking place in the neurones of variously etherized animals may be conveniently divided into four phases, as follows: In the first phase, the dendrites have lost their 'pyriform appendages' (the minute, very short branchlets which give them in some places their characteristic mossy appearance), and have

become quite smooth like the naked collaterals. In the second phase, the dendrites have become made up of short segments alternately transparent and opaque. In the third phase, many dendrites have become apparently sprinkled with a fine powder, or covered with very minute granulations, the axis cylinders and the collaterals as well often partaking in this change. Magnification of from 500 to 600 times is necessary to clearly bring this out in the early formation stage of the varicosities. In the fourth, or final phase, the varicosities are conspicuous on both the cylinder axes and the dendrites, especially in the cortex at the base of the brain, but also in more superficial regions of the cerebrum.

It has been conjectured by some that the formation of these little nodules is due to a mechanical contraction and coalescence of the finer projections of the neurone, but Mlle. Stefanowska, after the examination of thousands of carefully prepared specimens, emphatically concludes that "*the formation of the pearls on the dendrites and on the white fibers is due to a sort of liquefaction of the protoplasm under the influence of the agents which disturb the normal life of the neural cells.*" This is the important opinion of the article—that these varicosities are pathologic formations due to a more or less complete poisoning of the living protoplasm of which the neurones are composed. They are, it is concluded, droplets of liquified protoplasm. This is supported by the well-known fact that these 'pearls' may be produced after the death of the animal as well as before, and by poisoning agents other than ether. Again, the smaller varicosities produced by a slight etherization disappear only several days after they are produced by the action of the anæsthetic—much too long a time to allow the admission that it is a normal process, due to a functional retraction or contraction of the dendrites.

Anæsthesia seems, then, to the writer of the report a distinctly abnormal state, and the varicosities on the neuronal branches seem by no means to be accounted for on any theory of spontaneous neural ameboidism.

The Central Connections of the Auditory Nerve. ALDREN TURNER. Journal of Laryngology, Rhinology and Otology, XV. (3), March, 1900. Pp. 131–133.

We have here an account of experiments performed on the brains of monkeys which seem to add something to the knowledge of a part of cerebral anatomy at present even more obscure than it is important. This study of the conducting paths was carried out by means of the

well-known method of degeneration first largely used by Waller, while the tissues were prepared for the microscopic examination by Marchi's plan. The experiments were of three sorts, first, section of the auditory nerve distal to the auditory ganglion; second, by study of the degenerations following lesion of the accessory auditory ganglion; and third, destruction of the internal geniculate body.

Dr. Turner's own summary will best present the products derived from the experiments: "(1) The cochlear fibers end in the accessory auditory ganglion. (2) In the auditory ganglion the fibers of the corpus trapezoides arise, some passing through the lateral filet of the same side and some crossing to the opposite side. (3) The superior olivary bodies have a close and direct connection with the trapezoid body. (4) Fibers coming from the auditory ganglion pass directly to the internal geniculate body of the opposite side. (5) There is no evidence that fibers pass directly into the temporal lobe without interruption in the internal geniculate body. (6) The complementary corti-fugal tract of the auditory system is the so-called temporo-pontine tract of Bechterew."

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

NEW BOOKS.

Experimentell-psychologische Untersuchungen über das Urteil. K. MARBE. Leipzig, W. Engelmann. 1901. Pp. iv + 103.

The Wisdom of Passion. "SALVARONA." Boston, Mystic River Book Co. 1901. Pp. 248.

Das Problem des Wirkens und die monistiche Weltanschauung, mit besonderer Beziehung auf Lotze. MSCISLAW WARTENBURG. Leipzig, H. Haacke. 1900. Pp. 256.

Kants Theorie der Kausalität, mit besonderer Berücksichtigung der Grandprincipien seiner Theorie der Erfahrung. M. WARTENBERG. Leipzig, H. Haacke. 1899. Pp. viii + 294.

Public Worship. JOHN HYLAN. (Religion of Science Series.) Chicago, Open Court Publishing Co. 1901. Pp. 94.

Beiträge zur Akustik und Musikwissenschaft. Herausgeg. v. CARL STUMPF. 3. Heft. Leipzig, J. A. Barth. 1901. Pp. 147 + 11.

Dottrini di Rosmini sull'essenza della materia. C. GUASTELLA. Scuola Tip. "Boccone del Povero." 1901. Fasc. 1–2. Pp. 20+17.

Proceedings of the Aristotelian Society. New Series, Vol. I. London, Williams & Norgate. 1901. Pp. 239.

Introduction to Psychology. Miss M. W. Calkins. New York, Macmillans. 1901. Pp. xv + 509. Price, $2.

Dictionary of Philosophy and Psychology. Ed. by J. Mark Baldwin. In three volumes, I. and II., the text, sold with or without III., the Bibliographies. Vol. I., A to Law. New York and London, Macmillans. 1901. Pp. xxiv + 644. Large 8vo. $5 (21s), per volume.

Descartes' Meditations and Selections from the Principles of Philosophy (and other matters). Trans. by J. Veitch. Chicago, Open Court Co. 1901. Pp. xxx + 249.

Report of the (United States) Commissioner of Education, 1899–1900, Vol. I. Washington, Government Printing Office. 1901. Pp. lxxx + 1280.

Physik des Seelenlebens. J. Pikler. Leipzig, Barth. 1901. Pp. 40. M. 1.20.

Psychologische Untersuchungen zur Bedeutungslehre. Ed. Martinak. Leipzig, Barth. 1901. Pp. vii + 98. M. 3.

Panideal, Psychologie der sozialen Gefühle. R. Holzappel. Leipzig, Barth. 1901. Pp. 233. M. 7.

Begriffe und Theorien des Modernen Physik. J. B. Stallo. Trans. from the English, with Introduction by E. Mach. Leipzig, Barth. 1901. Pp. xx + 332. M. 8.50.

La Psychologie du Rêve. N. Vaschide and H. Piéron. Paris, Baillière. 1902. Pp. 95.

La Mémoire. J. J. van Biervliet. Bibl. intern. de Psychol. exper. Paris, Doin. 1902. Pp. 349. Fr. 4.

Reports of the Cambridge Anthropological Expedition to Torres Straits. Vol. II., Physiology and Psychology. Part I.: Introduction and Vision. W. H. R. Rivers. Cambridge, Univ. Press. 1901. Pp. vi + 132. Large 8vo. 9s.

NOTES.

Dr. Nicholas Murray Butler, professor of philosophy and education in Columbia University, has been made acting president of that University, in consequence of the resignation of President Seth Low.

The following appointments are announced: J. E. Lough, Ph.D. (Harvard), lately of the State Normal School, Oshkosh, Wis., to

the professorship of psychology in the School of Pedagogy, New York University; Frank Nicholas Spindler, as professor of psychology and pedagogy in the State Normal School, Stevens Point, Wis.; A. H. Yoder, of the University of Indiana, to the professorship of pedagogy in the University of Washington; E. B. Holt, M.A. (Columbia), Ph.D. (Harvard), as instructor of psychology in Harvard University, succeeding Dr. Robert MacDougall; W. J. Shaw (M.A., Toronto), formerly at Wesleyan University, as demonstrator in psychology at Princeton University; Dr. Earl Barnes, lately of Stanford University, as special lecturer in pedagogy in the School of Pedagogy, New York University; Dr. W. H. Sheldon, as assistant in philosophy at Columbia University.

DR. J. H. HYSLOP, professor of logic and ethics in Columbia University, has been given leave of absence for a sabbatical year; Dr. A. L. Jones, as lecturer, will take his courses. We regret to hear that Professor Hyslop is ill; he will spend the year in the Adirondacks.

DR. E. E. SCHIEB, of the University of South Carolina, has been appointed professor of philosophy and pedagogy in Tulane University; Dr. T. H. Haines has been appointed assistant professor of philosophy in Ohio State University.

WE note that the Sidgwick memorial is making satisfactory progress. It is decided to establish a Sidgwick Lectureship in moral science, open to women, at Cambridge. The American subscriptions are not as full as seemed reasonable to expect and it is hoped that further amounts will be sent to the undersigned before the close of the year 1901. An international committee has also been appointed to raise a fund for a statue of Auguste Comte to be placed in front of the Sorbonne, Paris. Subscriptions to this fund may also be sent to the undersigned at Princeton, N. J., for the committee.

J. MARK BALDWIN.

WE welcome the first number (July, 1901) of *Archives de Psychologie de la Suisse Romande*, edited by Th. Flournoy and Ed. Claparède, both of the University of Geneva. (Appearing at irregular intervals from the press of Eggimann, Geneva, and Alcan, Paris: Fr. 3.50 per number, Fr. 12 per volume.) We congratulate its editors upon its fine appearance and wish it a long and prosperous career.

Two other new journals are announced which should be of interest to psychologists: *Biometrica*, devoted to the scientific study of bio-

logical problems (already noted in these pages; the publishers are Cambridge University Press, Clay & Sons, London); and *Annalen der Naturphilosophie* (editor Ostwald; collaborators, Mach, Ratzel, J. Loeb, etc.; publisher, Fock, Leipzig), devoted to universal questions of epistemology (Erkenntnistheorie) and of scientific method. These journals both illustrate the union of scientific and philosophical enquirers.

BEGINNING November 1, 1901, all editorial matter for this REVIEW, together with books for notice, should be sent to Professor J. McK. Cattell, Garrison-on-Hudson, New York. Business communications should be addressed as heretofore to Professor H. C. Warren, Princeton, New Jersey.

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Stuart Professor of Psychology in Princeton University

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The first volume of the *Dictionary of Philosophy and Psychology* was issued in September. It has been in preparation for several years under the general editorship of Professor James Mark Baldwin of Princeton University. The writers comprise many of the leading scholars both in philosophy and science at home and abroad. Prof. Baldwin has associated with himself a board of Consulting Editors for each of the languages, English, German, French and Italian. The work may be described as a Dictionary with Encyclopedic Features. It is also a Dictionary of Science as well as of Philosophy, since it includes the principal terms of the Physical, Natural and Moral Sciences. Equal pains have been taken with the terms in all the positive sciences, so that the work will, it is hoped, be of value to scientific men generally. Such departments as biology, in which remarkable advances have been made in recent years, have been given much space, and the topics and recommendations as to terminology have been committed to the very highest authorities. It may be said, indeed, that in its scope and plan the Dictionary aims to sum up the results of scientific research, as well as of Philosophical thought, to the beginning of the twentieth century.

Among the new and special features of the work may be mentioned the recommendations, as to English terms, for all the topics in the dictionary with equivalents in three foreign languages—German, French and Italian. These recommendations have been arrived at by the united boards of editors, and so have all the weight of their special authority. Writers and translators, as well as readers who wish to be *au courant* with results in other languages as well as in English will find this feature of interest.

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